SOME DETERMINANTS OF PERCEIVED CONTROL

by

Camille B. Wortman

Department of Psychology
Duke University

Date: _____

Approved:

_____
Jack W. Brehm, Supervisor

_____

_____

_____

_____

_____

Dissertation submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in the Department of Psychology in the Graduate School of Duke University

1973

ABSTRACT

(Psychology-Social)

SOME DETERMINANTS OF PERCEIVED CONTROL

by

Camille B. Wortman

Department of Psychology
Duke University

Date: _____

Approved:

Jack W. Brehm, Supervisor

_____

_____

Edward E. Jones

_____

An abstract of a dissertation submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in the Department of Psychology in the Graduate School of Duke University

1973

ABSTRACT

SOME DETERMINANTS OF PERCEIVED CONTROL

by

Camille B. Wortman

Evidence was reviewed in support of the assertion that individuals are motivated to maintain effective control over their environment. Two types of evidence were considered: experiments indicating that control is adaptive, and studies suggesting that individuals believe they can control chance outcome.

Taken as a whole, these studies suggested the following hypothesis in regard to control: an individual will feel control over an outcome if he causes the outcome, and if he knows before causing it what he hopes to obtain.

An experiment was conducted to test this hypothesis. Subjects were asked to rate a series of consumer items. They were then told that the experimenter had a large supply of two of the items, and that they would get to win one by a chance drawing. Each subject was instructed to pick two colored marbles from a large bowl of such marbles and hand them to

the experimenter, who placed them in a coffee can and mixed them up. One-third of the subjects (Experimenter-Caused Foreknowledge condition) were told that the experimenter would pick a marble to determine their prize, and were told beforehand which marble stood for which prize. One-third (Subject-Caused Foreknowledge condition) were told to select a marble to determine their prize, and were told before picking a marble which marble stood for which item. The remaining subjects (Subject-Caused No Foreknowledge condition) were told to select a marble to determine their prize, but were not told until after they had picked their marble which marble stood for which prize. In all cases, the person picking the marble could not see into the can. Crosscutting this manipulation was an attractiveness manipulation: half of the subjects received a marble which led them to obtain an item they had rated high in attractiveness; the remaining subjects "won" an item they had rated low. Subjects were then asked to rerate the items, and ratings of perceived control over the outcome were obtained. Subjects were also asked to indicate feelings of choice and responsibility for the outcome. The results strongly supported the hypothesis: subjects in the S-Caused Foreknowledge condition perceived themselves to have significantly more control over the outcome than subjects in the other two conditions. These subjects also perceived themselves to have more choice about which item they received, and more responsibility for their outcome than subjects in the remaining conditions. As expected, a comparison of the E-Caused Fore-

knowledge and the S-Caused No Foreknowledge conditions revealed no significant differences. The attractiveness variable did not affect perceived control, choice, or responsibility. However, subjects who won the less attractive item viewed the procedure as more unfair and the outcome as more "fixed" than subjects receiving a highly attractive item. Surprisingly, the experimental treatments did not affect the subject's evaluation of the items. Subjects in the S-Caused Foreknowledge cell did not come to view their outcome more positively, even when they felt choice and responsibility for it. A second experiment is reported in which both foreknowledge and attractiveness were manipulated in a 2 x 2 factorial design. By this experiment, all subjects caused their own outcome. The results strongly supported those obtained in the first experiment. Subjects who caused their outcome and had foreknowledge concerning the consequences experienced more control, choice, and tended to experience more responsibility than subjects who caused their outcome but did not have foreknowledge. Again, there were no effects for outcome attractiveness, and the experimental treatments did not affect subjects' evaluations of their outcomes. The relationship between these studies and previous experiments on control was explored, and some possibilities for future research on control were considered.

ACKNOWLEDGMENTS

The author wishes to thank Jack W. Brehm, who provided a constant source of stimulation, guidance, and encouragement throughout all phases of this work. Thanks are also due to Albert Erlebacher and Gregory Kennington for their assistance in analyzing the data and to Paul Wortman for critical comments on the manuscript.

C. B. W.

CONTENTS

LIST OF TABLES

INTRODUCTION

Control. That's what current psychology is all about.

P. G. Zimbardo (1969)

The idea that man strives for mastery and control of his environment is certainly not new, and has played a central role in many theoretical statements. In his theory of constructive alternativism, Kelly (1955) emphasizes the importance of predictability and control:

> It is customary to say that the scientist's ultimate aim is to predict and control. . . . Yet, curiously enough, psychologists rarely credit the human subjects in their experiments with having similar aspirations. It is as though the psychologist were saying to himself, "I, being a psychologist, and therefore a scientist, am performing this experiment in order to improve the prediction and control of certain human phenomena; but my subject, being merely a human organism, is obviously propelled by inexorable drives welling up within him, or else he is in gluttonous pursuit of sustenance and shelter." . . . Might not the individual man, each in his own personal way, assume more of the stature of the scientist, ever seeking to predict and control the course of events with which he is involved? (p. 5)

This theme has also played a major role in the writings of Woodworth (1958), who has argued that men and animals strive to master their environments. Woodworth has suggested that a great deal of effort is directed toward producing effects on the environment, even when these effects do not aid in the satisfaction of basic needs.

In his classic paper on competence, White (1959) has suggested that many diverse behaviors, such as visual exploration, grasping, crawling, and walking, attention and perception, language and thinking, and exploring novel objects, have a common biological significance: "They all form part of the process whereby the animal or the child learns to interact effectively with his environment (p. 329)." According to White, individuals experience feelings of satisfaction or efficacy when their behavior produces changes in the stimulus field. De Charms (1968) has argued also that man is motivated to view himself as a causal agent. And in his recent review of attribution processes, Kelley (1971) has stressed the importance of control:

> The purpose of causal analysis--the function it serves for the species and the individual--is effective control. The attributor is not simply an attributor, a seeker after knowledge, his latent goal in gaining knowledge is that of effective management of himself and his environment. He is not a pure "scientist," then, but an applied one (p. 22).

Perhaps the most appealing thing about the idea that man is motivated to maintain effective control over his environment is its compatibility with a variety of theoretical formulations. For example, Zimbardo (1969) has discussed the relationship between cognitive consistency and control:

> It is frequently necessary to strive for consistency because consistency between action and self-knowledge, between word and deed, are so prized in our culture that to be inconsistent is to be abnormal. If one's behavior is not comparable to that of people whom he uses for reference, then he must convince his observing, critical self that his commitment follows <u>rationally</u> from an analysis of the stimulus conditions . . . The psychological homeostasis posited by such a consistency principle is not an end in itself, but rather a means toward

> minimizing dependency on the environment and maximizing control over it. This is achieved not by accepting the environment as given but by modifying it to effect a "rational" fit after the commitment (p. 239).

Zimbardo's argument that individuals attempt to structure reality so that their behavior will appear rational is quite similar to Bramel's (1969) reinterpretation of dissonance theory. Bramel has asserted that dissonance is aroused when one behaves incompetently or immorally, and his view is entirely consistent with the assertion that men are motivated to maintain effective control over their environment.

A second theoretical formulation that is compatible with this notion is Lerner's (1971) just world theory. Lerner has argued that men are motivated to believe in a "just world," where people get what they deserve, and deserve what they get. According to Lerner, the issue of deserving begins to gain meaning in the following way. During development, the child learns to forego immediate gratification and instead to endure deprivation, effort, and pain so that he may achieve desirable outcomes in the long run. According to Lerner (1971), "If the person becomes aware that someone else--who lives in and is 'vulnerable' to the same environment--has received undeserved suffering or failed to get what he deserved, the issue must arise as to whether the person himself can trust his environment (p. 8)." "If these things can happen, what is the use of struggling, planning, and working to build a secure future for one's self and family?" (Lerner, 1970, pp. 207-208) Since evidence

of an unjust world threatens a person's control over his potential outcomes, he will be motivated to distort reality so that the world appears just. In line with this reasoning, it has been found that individuals will derogate a person who has received a negative outcome, even though it has been made perfectly clear that the outcome occurred by chance (cf. Lerner & Simmons, 1966).

A third theory that is consistent with the notion that men are motivated to maintain effective control over their environment is Brehm's (1966, 1972) theory of psychological reactance. Brehm has maintained that when an individual's behavioral freedom is threatened he will become motivationally aroused to restore his freedom. According to Brehm, the magnitude of this motivational arousal is a direct function of the importance of the freedom, the proportion of freedoms that are threatened or eliminated, and the number of freedoms threatened by implication. This theoretical formulation has received support from a variety of experiments (see Brehm, 1972, for a review). Both the theory and the supporting experimental work are consistent with the notion that man is motivated to maintain effective control over his environment.

In summary, psychologists have been theorizing about control for years, and their speculations appear compatible with dissonance theory (at least as viewed by Aronson, 1968; Bramel, 1968; and Zimbardo, 1968, 1969), the just world theory, and reactance theory. Recently, a number of individuals have conducted experimental work in this area.

Those experiments that bear directly on the theoretical statement set forth will be reviewed, with a view toward elaborating, refining, and extending the statement that men are motivated to maintain effective control. The following types of evidence will be considered: (a) evidence in support of the notion that control is adaptive--that it aids the performance of the organism, and reduces the adverse effects of stressful stimuli; and (b) evidence--both experimental and anecdotal--that under certain circumstances individuals believe that they control chance outcomes, and make active attempts to influence fate, chance, and luck.

Control is adaptive

One problem in considering the experimental literature on control is that there are almost as many definitions of control as there are experiments. Some experimenters (see, for example, Geer, Davidson, & Gatchel, 1970; Glass & Singer, 1972) have led their subjects to believe that they are "in control" because, by making the correct instrumental response, the subject can avoid an aversive stimulus. These experimenters implicitly define control as the self-attribution of success. In the experiment by Geer, Davidson, and Gatchel (1970), subjects were told that the experiment concerned reaction time. They were first put through a reaction time task in which they were told to react to the onset of a 6-second shock. It was explained to them that the shock would always be 6 seconds, regardless of how fast their reaction time was.

After 10 trials at this preliminary task, half of the subjects were told that, if they decreased their reaction time, the shocks would be reduced. The remaining subjects were merely told that the shocks would be reduced. All subjects received shocks of 3-second duration during the second part of the study. There was no check on the control manipulation--subjects were not asked how much control they felt they had over their shocks. However, subjects who were told that their reaction times could reduce the shocks showed smaller skin conductance responses to the shocks, and exhibited fewer skin conductance fluctuations. These results were taken to indicate that Perceived Control subjects experienced less physiological arousal when subjected to the second set of shocks than did No Perceived Control subjects.

In an experiment by Glass and Singer (1972), all subjects were told that they would be exposed to loud noise, and that by successfully solving a series of puzzles they would be able to avoid some of the noise bursts. Half of the subjects (Perceived Avoidance condition) received primarily soluble puzzles; the remaining subjects (No Perceived Avoidance condition) were given insoluble puzzles. Both groups received the same number of noise bursts. All subjects received a soluble puzzle on the last trial. The experimenter recorded the amount of time it took subjects to reach a solution on this puzzle in order to obtain a measure of active problem-solving ability. In addition, GSR responses were obtained during the noise bursts. After completing the final puzzle, subjects were asked to

complete a proofreading task and the Stroop Color Word Test in order to measure possible performance decrements. They were also asked several questions designed to assess the effectiveness of the control manipulation.

The questions designed to assess the effectiveness of the control manipulation revealed that it was indeed successful. Subjects were asked to indicate to what extent they felt they could have controlled the occurrence of the noise bursts. Perceived Avoidance subjects reported greater feelings of control than No Perceived Avoidance subjects. The GSR results revealed that both groups of subjects adapted to the noise. However, subjects in the No Perceived Avoidance condition took longer to solve the final puzzle than subjects in the Perceived Avoidance condition, suggesting that active problem solving declines when subjects cannot control the noise. The aftereffects also appeared to be more serious for subjects who did not have control of the noise. Subjects who could not control the noise performed worse on the proofreading task and the Stroop test than subjects who could control the noise.

These results were essentially replicated by Glass and Singer (1972) using electric shock as the aversive stimulus. Glass and Singer realized that their noise experiment was open to the following alternative explanation: maybe it was working on insoluble puzzles, rather than being subjected to unavoidable noise, that resulted in the performance decrement. Working on so many insoluble puzzles may have frustrated

subjects in the No Perceived Avoidance condition, and they may have tried less hard on the post-noise tasks. In order to rule out this alternative explanation, Glass and Singer added two control groups to the present experiment--one worked on primarily soluble puzzles, and one worked on insoluble puzzles, but neither was subjected to electric shock. Except for the addition of these groups, and the use of electric shock instead of noise, the experiment was quite similar to the one described above. The results indicated that the performance on the proofreading task and the Stroop test was still better for subjects who felt they avoided electric shock by working the puzzles correctly than subjects who did not have this perception. There was no difference in performance for the control conditions, ruling out the alternative explanation mentioned above. The results from the Perceived Avoidance subjects were found to be similar to those of the soluble control subjects.

In addition to completing the proofreading and Stroop tests, subjects in this experiment were asked to fill out a postexperimental rating scale. This scale contained several adjective pairs, and subjects were asked to indicate which was most characteristic of them. The results revealed a significant difference between the Perceived Avoidance and No Perceived Avoidance conditions on the adjective pairs, "helpless-confident," "strong-weak," and "competence-incompetence," with subjects in the No Perceived Avoidance condition viewing themselves as more helpless, weak, and incompetent.

Glass and Singer were still concerned that a "frustration" hypothesis could be applied to their data. As they point out, it may be argued that subjects in the No Perceived Avoidance condition became frustrated as they continually tried and failed to work the puzzles, and it may have been this frustration rather than lack of control that led to adverse aftereffects. In order to check on this possibility, they conducted an experimental treatment in which subjects worked on insoluble puzzles and were subjected to shocks, but were told that their success or failure on the puzzles would have nothing to do with the amount of shocks they received. Surprisingly, when asked about perceived control over the shocks, this new group reported marginally less control ($p < .10$) than subjects who thought the shocks occurred because of their failure to solve the puzzles. Contrary to the predictions based on the notion of frustration, this new group did just as poorly on the proofreading task and the Stroop test as the subjects who thought the shocks occurred because of their poor performance. Glass and Singer play down the marginal difference in perceived control. They argue that, since there is no difference in the post-noise performance of those two groups, the results support a perceived control, rather than a frustration, explanation.

Taken as a whole, these studies indicate that when control is defined as the belief that one's response terminates an aversive stimulus, subjects who believe that they have controlled the stimulus show fewer decrements in performance than those who do not. They also report

themselves to be more confident, strong, and competent than subjects who do not. The experiment by Geer et al. (1970) suggested that subjects who believe their responses control an aversive stimulus show less autonomic reactivity than subjects who do not, although this finding was not demonstrated in the experiments conducted by Glass and Singer. In the opinion of the current author, however, these studies add little to our conceptual understanding of control. It can be argued that the manipulation of control is little more than a manipulation of success. It has already been demonstrated that subjects who succeed at one task will perform better on other tasks (cf. Lewin, Dembo, Festinger, & Sears, 1944). And the fact that these subjects view themselves as more confident, strong, and competent than subjects who have not had a success experience is hardly surprising. Of more theoretical interest is the comparison made by Glass and Singer (1972) of subjects who believed they were shocked because of their failure to solve puzzles, and the subjects who worked on the same insoluble puzzles and received the same shocks, but were led to believe that the shocks were not related to their performance. As noted previously, subjects in the former condition reported marginally greater feelings of control, but performed no differently on the proofreading or Stroop tests. Of course, a comparison of these two groups cannot be considered to be conclusive, since the second group was run after a study including the first group was completed. A replication is necessary before we can determine whether this difference

suggested by Glass and Singer's data is reliable. If this turns out to be the case, Glass and Singer's arguments concerning the ameliorative effects of perceived control would suggest that the failure subjects will perform better on post-noise tasks than the subjects who think their performance is unrelated to the shocks. If failure subjects do report greater feelings of control but do not show the ameliorative aftereffects characteristic of Glass and Singer's subjects, we will have demonstrated a condition under which perceived control is not adaptive.

In the studies described previously, subjects were led to believe that they had actually exercised control over the aversive stimulus. Other experiments on this topic have placed subjects in a position of having potential control over the stimulus. Subjects in experiments of this type are typically told that there is a button that they can press that will terminate the shocks, but that the experimenter would prefer that they not use it. For example, Corah and Boffa (1970) conducted an experiment in which subjects were given 10 no-escape trials from aversive white noise. All of the subjects were told that the button in their right hand would turn off the sound. However, they were told that they should not press this button. For half of the subjects, it was added that, if the sound was so uncomfortable that they felt they must turn it off, they could press the button. The experimenter pointed out that the choice about pressing the button was up to them. In these no-escape trials, Corah and Boffa (1970) found GSR magnitude to be significantly higher

when the subject had no potential control over the shock--i.e. when he was instructed not to press the button and was given no choice about pressing it. There was also a marginally significant effect for ratings of discomfort--subjects who were given a button and told not to press it rated the shock as creating marginally more discomfort than subjects who were given free choice about pressing the button.

This experiment was criticized by Glass and Singer (1972), who point out that the study

> did include a pair of conditions that could be interpreted as simply giving the subject a sense of control or no control, but the way in which the variable was included may have artifactually maximized GSR amplitude; that is, No Control subjects were given a button that could presumably terminate shock and were told not to press it. It is entirely possible that bringing up the issue of controllability in the No perceived Control Condition heightened the subject's subsequent frustration at being unable to escape from shock; hence, increasing the magnitude of autonomic response (p. 35).

Glass and Singer (1972) have conducted two experiments using this type of control, and have designed their studies to eliminate the possible artifact present in the Corah and Boffa (1970) experiment. In each of their studies, subjects were subjected to a tape containing several loud random-intermittent noises. Half of the subjects were led to believe that they had potential control over the noise. They were shown a switch attached to the side of their chair and told, "Now this button affects the noise you will be hearing while you work on the tasks. You can terminate the noise by pressing the button . . . Of course, whether or not you press it is up to you . . . We'd prefer that you do not, but that's entirely

up to you." Only four subjects in the two experiments exercised this option, and their data were not included in the analysis. The other half of the subjects were shown no button and control was not mentioned.

After listening to the noise, subjects were asked to complete a proofreading task in order to measure post-noise performance decrements. They were also given two insoluble puzzles. The number of trials that subjects worked on these puzzles was taken to be an index of tolerance for frustration. After completing these measures, subjects were asked some additional questions designed to check on the effectiveness of the manipulation.

On a scale ranging from "no control at all" to "complete control," subjects were asked to indicate to what extent they felt they really could have had the noise stopped. In both experiments, subjects in the Perceived Control condition viewed themselves as having significantly more control than subjects in the No Perceived Control condition. Subjects' responses to the insoluble puzzles indicate that perceived control had ameliorative effects on tolerance for frustration, and data from the proofreading task indicate that in both experiments the mean percentage of errors missed was greater for subjects in the No Perceived Control condition than for subjects in the Perceived Control condition ($p < .01$ in the first experiment and $p < .08$ in the second). On the basis of these findings, Glass and Singer concluded that "perceived control appears to reduce the aftereffects of unpredictable noise to a point where they

resemble performance following predictable noise, or no noise at all (p. 67)."

After completing these experiments, Glass and Singer became concerned that a rather trivial alternative explanation could account for the results. They pointed out that perhaps the experimenter is viewed as a nicer person, or as a more human or competent experimenter, in the Perceived Control condition than in the No Perceived Control condition. If this were the case, responses on the puzzles and proofreading task might simply reflect the subjects' greater desire to please the experimenter in this condition. In order to check on this possibility, they added questions concerning the subject's opinion of the experimenter in a later study (pp. 74-78). The results of that study were consistent with the results from their other studies. Subjects in the No Perceived Control condition made significantly more errors on the proofreading task than subjects in the Perceived Control condition. In addition to the proofreading task, subjects were given an "anonymous" questionnaire in order to ascertain their opinion of the experimenter, and their opinion concerning the fairness of the experimental procedure. Subjects were found to like the experimenter about the same in the two conditions. When asked to indicate whether the way in which the experiment was conducted was fair or unfair, subjects in the No Perceived Control condition tended to view the session as less fair than subjects in the Perceived Control condition, but Glass and Singer do not report a test of significance for this

difference.

These experiments provide cogent evidence that possession of control over aversive stimuli is adaptive. Glass, Reim, and Singer (1971) reasoned, however, that it is rare to have direct control over aversive stimuli in "real life." They maintained that individuals often have a kind of indirect control--they can complain to public officials in the hope that this will effect the occurrence of the aversive stimuli. Glass, Reim, and Singer were interested in whether the perception of access to a representative who has control over aversive events would actually result in the perception of control. Furthermore, they hoped to determine whether such indirect control would act like direct control in reducing the negative aftereffects of an aversive stimulus.

Glass et al. (1971) designed a clever experiment to determine subjects' reactions to this kind of indirect control. All subjects entered the lab individually, and were introduced to a second subject who was actually a confederate of the experimenter. Subjects received instructions that they would be subjected to a series of loud noises. Subjects in the Perceived Indirect Control condition observed as the confederate was given a switch that would terminate the noise if he chose to press it. Although he was not given a control button, the subject was given a switch and told that, if he wished the noise to be terminated, he could signal the other subject. After pointing out this switch to the subject, the experimenter turned to the confederate and said: "When the light is

activated, this means Mr. (name of subject) would like you to press the control button . . . Whether or not you press the button is up to you." Each subject was then told that the experimenter would prefer it if he did not signal the other subject, but that "the decision is up to you." In addition to this Perceived Indirect Control condition, a number of other conditions were run for comparison: A No Perceived Indirect Control condition, in which subjects were led to believe that the other subject had a control button but were given no opportunity to communicate with him; a Together-No Perceived Control condition, in which neither subject could control the noise; and an Alone-No Perceived Control condition, in which the subject was exposed to the noise alone and without any means of terminating it. After the experimental treatment, all subjects were asked to take the proofreading task and a measure of quality of performance was obtained. Subjects were also asked questions designed to check the effectiveness of the control manipulation. The results revealed that subjects in the Perceived Indirect Control condition perceived themselves to have significantly more control over the noise than subjects in each of the other conditions. And, as predicted, subjects in this condition made significantly fewer errors than subjects in the No Perceived Control condition ($p < .01$), the Together-No Perceived Control condition ($p < .01$), and the Alone-No Perceived Control condition ($p < .07$).

These studies provide convincing evidence that perceived potential control over an aversive stimulus reduces the negative aftereffects that

are likely to occur as a result of the stimulus. The theoretical implications of the experiment by Glass et al. (1971) are particularly interesting, since subjects in the Perceived Indirect Control condition were given no assurance that the person in charge would pay attention to their request. Their results suggest that the mere possibility of control is effective. In future research it would be interesting to vary the probability that the person "in control" will respond positively to the subject. Is a very slight possibility of control enough to result in ameliorative effects, or must the subject be relatively certain that his indirect control will produce results?

So far, it has been established that two types of control are effective in reducing the aftereffects of an aversive stimulus: the belief that one's instrumental responses have terminated the stimulus, and the belief that one can press a button if one wishes that will either terminate the stimulus or alert another to do so. In addition to these manipulations, a third type of control has received some attention. Several investigators have defined control as the self-administration of an aversive stimulus. Subjects in experiments of this type are typically asked to administer shock to themselves at certain times, and their reaction to these shocks is compared to that which occurs when the shock is administered by the experimenter. In some ways, these studies are the most theoretically interesting of all the experiments on control. It could certainly be argued that subjects have no real control over the shock in either

case. Given that the subject is going to receive a certain number of shocks, and has no choice about their intensity or duration, what difference should it make who presses the button that turns on the shock? Does pressing the button and thereby causing the shock to occur lead to the perception of control? Are self-caused aversive outcomes less anxiety-producing than those caused by others?

Unfortunately, the majority of experiments using this type of control have methodological weaknesses that make it impossible to draw meaningful conclusions from the results. LePanto, Moroney, and Zehnhausern (1965) conducted an experiment in which subjects were subjected to radiant heat, and were asked to indicate whether it was painful. For half of the trials, subjects could turn off the heat; in the remaining trials, subjects' response turned off a clock and the experimenter turned off the stimulus as soon as he saw the clock go off. In support of the authors' reasoning that control over stimuli reduces pain, subjects who could turn off the stimulus themselves showed higher pain thresholds for the radiant heat than subjects who did not. Although the details of the method are rather skimpy, it seems reasonably clear that subjects were exposed to the stimulus for a shorter duration on these trials, since in the other trials the experimenter "turned off the stimulus with a delay equal to his reaction time (p. 475)." Therefore, it is not very surprising that subjects were willing to take more of the stimulus in the condition where they had control than in the condition where they did not.

In a frequently cited experiment by Haggard (1946), subjects entered the lab and relaxed while initial autonomic measures were taken. Then half of the subjects were informed that they would be shocked during the session, but were not told when the shocks would occur. The remaining subjects were told that a light would occasionally appear, and that when this happened they were to press a plunger. Subjects in this condition were told that pressing the plunger would turn on a shock. Haggard found that subjects who administered the shocks to themselves had weaker GSR deflections than subjects who had the shocks administered by the experimenter. However, it is not clear whether this result occurred because of the difference in who administered the shock. Unfortunately, subjects who administered the shocks themselves could predict when it would occur, while subjects in the Experimenter-Shock condition could not. A similar confounding of predictability and subject versus experimenter administration of the aversive stimulus occurs in an experiment by Staub, Tursky, and Schwartz (1971, Experiment II). In this study, one group of subjects was permitted to control the onset, intensity, and duration of shocks. A yoked group of subjects received the same shocks but had no such control. Subjects who were permitted to have control were able to endure more shocks than the yoked subjects. Again, it is not clear whether it was the control over the administration of the stimulus, or the ability to predict the occurrence of the stimulus, which produced this finding.

A confounding of predictability and control is also present in the first of two experiments reported by Hokanson, DeGood, Forrest, and Brittain (1971). In this experiment, all subjects were subjected to a stressful shock-avoidance procedure. Half of the subjects were permitted to take rest periods whenever they wished; a yoked group of subjects received the same rest periods but had no control over their onset. The group that could control (and also predict) the rest periods showed substantially reduced systolic blood pressure levels; their yoked counterparts did not. In a second experiment, the authors attempted to control for predictability. All subjects were subjected to an aversive shock-avoidance procedure, and were permitted to take rest periods whenever they wished. Interspersed between their rest periods were an equal number of rest periods determined by the experimenter. Both were preceded by reliable signals. The measure of systolic blood pressure reduction revealed a marginally significant effect for the control variable ($p < .10$).

Another experiment dealing with this type of control has been conducted by Pervin (1963). Pervin's study employed a within-subjects design, and predictability and control were varied independently. In the Control conditions, the subject pressed a lever which produced shock if shock was to occur on that trial. In the No Control conditions, the subject pressed a lever, but the lever that controlled the shock was pressed by the experimenter. Subjects were instructed to press the lever as soon

as the signal went off. The relationship between the signal light and the shock was used to manipulate predictability; sometimes it was clear from the signal whether or not shock would occur; sometimes an unreliable signal was used; and sometimes it was impossible to predict the shock from the signal.

Thus, there were six conditions in all. Each subject was exposed to all possible pairs of conditions and asked to indicate which he preferred. Subjects were found to prefer the predictable shock to the unpredictable shock. Consistent with the assertion that individuals are motivated to maintain control, subjects also showed a strong preference for administering the shock themselves rather than having the experimenter administer it. Unfortunately, Pervin (1963) did not provide a check on his control manipulation by asking subjects whether they indeed perceived more control in the Subject-Caused condition than in the Experimenter-Caused one.

Another experiment that bears on this discussion has been conducted by Stotland and Blumenthal (1964). In this study, subjects did not actually determine some aspect of the stimulus, but expected to do so in the future. Subjects were led to believe that they would be taking a series of tests to measure important abilities and skills. Half of the subjects were informed that they could take the tests in any order they wished. The remaining subjects were told that they must take the tests in a specific order "for administrative purposes." It was emphasized to both

groups of subjects that "it has been found that the order in which you take the tests does not make any difference in the scores (p. 141)." Anxiety was measured by changes in the palmar sweating. The results revealed that subjects who had no control over the order of the tests increased in palmar sweating, while those who had control did not change.

All of the experiments reviewed in this paper have involved the use of human subjects. It should be pointed out, however, that there is an extensive animal literature demonstrating the importance of perceived control in reducing the stress associated with an aversive stimulus. These studies were not considered here primarily because they have been insightfully reviewed elsewhere (see Seligman, Maier, & Solomon, 1971). However, one set of animal studies does deserve comment--the work of Seligman and his associates on the phenomena of "learned helplessness."

Several studies by Seligman and his colleagues have compared the responses of naive dogs to those of dogs who have previously been exposed to uncontrollable electric shocks. According to Seligman, a naive dog behaves in the following manner when he is placed in a shuttle-box to receive escape avoidance training: When the shock begins, the dog runs frantically about, until it accidentally scrambles over the barrier and escapes the shock. On the second trial, the dog crosses the barrier even more quickly. This pattern continues until the dog learns to avoid the shock completely. However, Seligman points out that there

is a striking difference between this behavior pattern and that exhibited by dogs who have previously received inescapable shocks. According to Seligman (1970),

> A typical dog which has experienced uncontrollable shocks before avoidance training soon stops running and howling and sits or lies, quietly whining, until shock terminates. The dog does not cross the barrier and escape from shock. Rather, it seems to give up and passively accepts the shocks. On succeeding trials, the dog continues to fail to make escape movements and takes as much shock as the experimenter chooses to give (p. 4).

Another peculiarity in the responses of these dogs has also been mentioned by Seligman. He points out that, although they occasionally jump over the barrier early in the training session and escape, they generally fail to profit from exposure to the barrier-jumping shock-termination contingency. Instead of repeating this behavior in the future, they simply revert back to taking the shock. This behavior pattern contrasts markedly with that of naive dogs, who are quick to learn of such a contingency once it has been accidentally discovered.

In an interesting theoretical paper, Seligman (1970) suggests that the data from the study on learned helplessness in animals may provide a model of reactive depression in man. Seligman argues that, just as learned helplessness is caused by experience in which responding is independent of one's reinforcers, reactive depression may also have its roots in feelings of loss of control over one's outcomes. Seligman argues that, in both man and animals, lack of control induces a state of helplessness in which there is an absence of incentives for initiating strategies

designed to escape from the aversive stimulus.

Consistent with the notion that control is adaptive, Seligman and Maier (1967) have provided evidence that learned helplessness can be prevented by immunizing experiences with control over aversive stimuli. These investigators found that if dogs are given trials of escape-avoidance training before treatment with inescapable shocks, they do not become helpless. After receiving exposure to inescapable shocks, these dogs escaped and avoided shocks normally when returned to an escape-avoidance situation. Seligman and Maier argue that this result occurs because of the controllability of the initial shocks, since a yoked group who had initial experience with equivalent amounts of uncontrollable shocks did not show this effect.

Additional evidence in support of the notion that control is adaptive comes from Seligman's proposed "treatment" of his helpless dogs. Seligman, Maier, and Geer (1968) have found that helplessness can be cured by "forcing" an organism to recognize that there is a contingency between his behavior and his reinforcements. These investigators found that forcibly dragging the dog from side to side in the shuttle-box, and thereby confronting him with the contingency between his behavior and the shock termination, was sufficient to cure helplessness. According to Seligman, the recovery was complete and lasting: after being pulled across the center of the shuttle-box several times, each dog began to respond on his own. When the barrier was replaced, the dog continued

to escape and avoid. Taulbee and Wright (1971) have reported a similar "therapy" to be quite successful in the treatment of depressed humans. Their therapy involves putting each patient in a room where he is abused. A patient may be criticized for engaging in a certain behavior; when he ceases the behavior, he may be criticized for stopping. This treatment continues until the depressed person expresses hostility or anger. As soon as he does so, he is removed from the room. Taulbee and Wright argue that this technique is quite effective in curing depression. Like Seligman, these investigators forced the individual to emit a response which resulted in a change for the better in his environment. Theoretically, the patient then becomes aware that he can be effective in controlling his environment.

Taken as a whole, the studies considered so far provide supporting evidence for the assertion that control is adaptive. Although the operational definition of control has differed from experiment to experiment, past research has consistently shown that subjects who have perceived control over an aversive stimulus show fewer decrements in performance, and greater tolerance for frustration following exposure to aversive stimuli than subjects who do not. There is some evidence that subjects experience less autonomic reactivity to stressful stimuli when they perceive themselves to be in control. When given a choice, subjects choose a condition in which they control the administration of the stimulus, rather than the condition in which administration is controlled by

the experimenter. Organisms who receive training in which they control aversive stimuli fare better when subjected to uncontrollable stimuli than organisms who have had no such training. Trained animals do not exhibit the pattern of behavior of "giving up," called "learned helplessness" by Seligman (1970). And organisms who have become helpless or depressed can be "cured" if they are forced to emit behavior that changes their environment, and are thus confronted with the evidence that they can control their environment.

There are a few additional sources of information suggesting that control is adaptive. Some evidence comes from a study conducted by Janis (1958) on the relationship between preoperative fear and post-operative adjustment following surgery. Janis classified a large group of hospital patients according to how fearful they said they were. He found that the subjects who were the least successful in adapting to the operation and its resultant stresses were those who had exhibited low fear. Janis argued that the low-fear individuals succeeded in eliminating anticipatory fear by means of blanket denials of personal vulnerability. According to Janis, these denials were shattered when the patients experienced the severe pains that typically accompany and follow surgery. The patients who held these attitudes "were more likely than the others to display intense anger reactions on the day of the operation, to develop unfavorable attitudes toward the surgeon, and to experience sustained emotional disturbances (p. 358)." Janis maintains that these negative

effects occur because the shattering of a low-fear person's "blanket immunity" gives rise to a profound feeling of helplessness. He suggests that this situation may be avoided by giving hospital patients information that will maximize their feelings of control over the situation:

> Of special importance are those recommendations which help build up a sense of active control by informing the person about overt actions he can execute (e.g. how to move around the bed in such a way as to minimize muscular aches and pains), and about decisions that will be left up to him (e.g. how to judge when to request a sedative (Janis, 1958, p. 383).

Similar advice is offered by Kubler-Ross (1969) in her book on the process of dying. Kubler-Ross has suggested that the dying patient will be able to adapt better to this very stressful situation if he is given a sense of control over his environment. For example, she recommended to the wife of one patient to allow him to control the nature of their visits, and reports that "as soon as it was left up to him to set the time and length of the visits, they became brief but pleasant encounters (p. 49)." She reports that the nursing staff also attempted to increase the patient's feelings of control

> by allowing him to control certain times for infusions, changing bed sheets, etc., and not surprisingly perhaps, --he chose approximately the same number of times for these procedures as they had been previously done, with no anger and struggles involved. His wife and daughter enjoyed their visits more . . . [with this man] who had been difficult to live with when he was well, but who became almost unbearable when he was in the process of losing his controlling grasp on the environment (p. 49).

Additional evidence suggesting that giving individuals a feeling of control may be an effective means of reducing violence and hostility is

provided by a number of events that have been occurring in today's prisons. Most prison officials would agree that prisons are characterized by violence and aggression, and many institutions have been plagued with bloody riots and strikes. A number of administrators have suggested that this violence could be reduced if prisoners were given more feelings of freedom and control. In a few prisons, experimental programs have been instituted toward this end. For example, Farrell (1971) reports on reforms being tried at Walla Walla, a maximum security prison in the state of Washington. Under these reforms, an inmate was "permitted to dress as he wished, wear a beard, long hair or even an earring . . . decorate his cell to his own tastes and in other small ways maintain a semblance of individuality and freedom (p. 35)." Since the reforms have been instituted, the prison warden has reported "a noticeable lack of tension in there, almost a friendliness (p. 41)." Of course, conclusions concerning the effect of perceived control or freedom in prisons cannot be drawn until the proposition has been tested experimentally, or at least quasi-experimentally.

In addition to the conclusion that control is adaptive, one might infer from the behavior of Janis' hospital patients, Kubler-Ross's dying patients, and men in prisons throughout the country that lack of control results in anger, aggression, and violence. This possibility raises some questions that may be important for a theoretical understanding of control. First, it is important to know whether anger, aggression, and

violence occur as a means of regaining control over the environment. Lack of control has received almost no attention as a possible cause of aggression.[1] However, a number of individuals have advanced theoretical notions concerning aggressive behavior that can be interpreted within the framework of perceived control. For example, Zimbardo (1969) pointed to the increase in a large number of aggressive acts such as apparently unmotivated murder, child beating, and senseless acts of destruction. Zimbardo argues that the state of affairs exists because the social conditions of America today destroy individual identity by making people feel anonymous. He maintains that because of these feelings of anonymity people do not feel personally responsible for the consequences of their behavior, and people are not concerned with the evaluation of their behavior by others. "If others can't identify or single you out, they can't evaluate, criticize, judge, or punish you; thus, there need be no concern for social evaluation (p. 255)." Zimbardo has suggested that the anonymity characteristic of life today has produced a change in the restraints which normally inhibit aggressive and anti-social behavior, and has resulted in a situation in which behavior is freed from "obligations, liability, and the restrictions imposed by guilt, shame, and fear (p. 284)." It is entirely possible that people engage in violent and destructive acts not because restraints have been reduced, but in order to

---

[1] One exception is Worchel's (1969, 1971) work on the relationship between aggression and restrictions of freedom.

establish themselves as a causal agent, as a controlling force--in order to establish evidence of contingency between their own behavior and responses in the environment. Of course, there is likely to be a strong relationship between feelings of anonymity and feelings of lack of control, and the current author's ideas concerning violence and destruction are closely related to Zimbardo's. The difference is that Zimbardo never says that people feel less anonymous after engaging in acts of violence and destruction. He maintains that individuals behave violently because this behavior is pleasurable in itself (see p. 252), and because of a reduction in society's restraints--not in order to establish themselves as a causal agent or controlling force.

Fromm (1972) has also pointed to the upsurge in aggression and violence in America today. Fromm attributes these phenomena to increased feelings of boredom among the American people. However, much of the evidence that he cites is consistent with the notion that individuals engage in violence because it brings them feelings of control. For example, Fromm mentions the work of Harold Esler with delinquents, and points out that

> those adolescents seem to have acted criminally because it was the only way to . . . "make a dent." Some, reporting their experiences of stabbing or killing people, described a feeling they had never had before. They had felt the excitement of making somebody respond to them; the response was the victim's anguished face, and his groan of pain (p. 86).

It seems clear that a number of violent and anti-social acts, thought to be motivated by other causes, may be motivated by the desire for

control. For example, in a review of some recent investigations concerning rape, Schultz (1972) found that

> several studies show that virtually all rapes are planned and are not the results of hot flashes of passion. They also show time and again that the rapist is not motivated by sex but by an impulse to control and degrade his victim (pp. 61-62).

Another type of violent behavior that may be understood in terms of control is the recent upsurge in assassinations and mass killings. In the past few years, a large number of public figures, including Medgar Evers, Martin Luther King, Malcolm X, John and Robert Kennedy, George Lincoln Rockwell, and George Wallace, have been shot. Are assassins motivated by a desire to establish themselves a cause? The comments of some of these individuals suggest that this is at least a possibility. Arthur Bremer, the man who shot George Wallace, made several statements to the effect that finally his behavior had "made a difference to someone" and that he was now "part of the world's history (cf. Buresh, 1972, p. 24)."

If this reasoning is correct, the implication is that hostility and aggression can be reduced if people are given feelings of control over their environment--if they are confronted with evidence of a contingency between their behavior and environmental events. Unfortunately, there is not a great deal of research that is concerned with this topic. However, a study by Thibaut, Coules, and Robinson (reported by Thibaut & Riecken, 1955) provides some evidence relevant to this assertion. Subjects were students in three sections of an introductory psychology

course and were told that the experiment was concerned with the effectiveness of college teaching. A guest lecturer was introduced, and subjects were asked to make a first-impression rating of the lecturer. Then the lecturer began a prepared speech that was intended to arouse hostility among the students. In this speech, he accused the students of being narrow and uncultured, and of lacking sensitivity and psychological insightfulness. The subjects were instructed to jot down criticisms of the speaker during this time. As soon as the speech was over, these criticisms were collected. In one of the sections, the guest lecturer read through the criticisms and announced that he would try to take them into account in his subsequent presentation. In the second section, the lecturer read the criticisms, but announced that he would not change his subsequent behavior to accommodate the suggestions. In the third section, the regular instructor collected the criticisms and informed the subjects that they would be used for research purposes and that the guest lecturer would not see them. The lecturer then resumed his talk for 10 additional minutes. The final portion of his talk was designed to be inoffensive. After this 10-minute period, students were asked to evaluate the lecturer again.

The results were consistent with the notion that perceived control over the environment reduces aggression and hostility. In the group of subjects who were told that their comments had been effective in changing the speaker's behavior, the majority of students did not rate the lecturer

more negatively the second time than they had the first. However, of the subjects who were told that the speaker had chosen to ignore their comments, the vast majority of them became more hostile in their attitudes toward the lecturer.

Of course, this study has a number of obvious methodological weaknesses and should be replicated before we conclude that perceived control is effective in reducing aggressive impulses. However, another theoretical question should be raised at this point. As noted previously, the work of Janis (1958) and Kubler-Ross (1969), as well as the events occurring in many of today's prisons, suggests that lack of control leads to anger and aggression. In contrast, the research and theoretical work by Seligman and his colleagues suggest that exposure to uncontrolled outcomes will lead an individual to become passive and depressed. How can these two points of view be reconciled?

The problem in understanding how individuals will react to loss of behavioral control is exemplified in a recent study by Roth and Bootzin (in press). The purpose of their experiment was to demonstrate the existence of learned helplessness in humans. There were two phases to the experiment. In Phase I, the subjects were induced to work on problem-solving tasks, and their expectancies of control were manipulated. One group ("helpless") received random reinforcement for performance on a problem-solving task; a second group ("double helpless") received random reinforcement on two problem-solving tasks

administered consecutively. There were two control groups: one that received contingent reinforcement during the problem-solving task in Phase I; and a no-treatment control group that did not participate in Phase I. After completing the problem-solving task, subjects reported to Phase II, which they were led to believe was a separate experiment.

In Phase II all subjects were presented a series of concept formation problems on a TV monitor, and were required to solve 25 consecutive problems correctly. The experimenter induced an equipment malfunction on every 10th trial (by blurring the monitor screen) and thus prevented the subject from completing his task. There were three ways that a subject could respond to this dilemma. He could make a guess on the trial even though he could not see the stimulus. If a subject adopted this strategy, he was given feedback which alternated between "right" and "wrong," making it impossible for him to get 25 items correct in a row. Or, he could get up and get the experimenter. Subjects who selected this strategy were told by the experimenter that she would check the equipment, and the malfunction was temporarily fixed. It occurred again on every 10th trial, however, but was "fixed" as soon as the subject stood up to go for help. A third possible response was to just sit there. If subjects chose this strategy, the blurred stimulus was left on the screen.

Roth and Bootzin were interested in whether subjects' responses to this aversive outcome would be affected by their prior training. On the

basis of Seligman's work, they predicted that subjects who had received training which demonstrated a lack of correlation between their behavior and their reinforcement would become helpless in Phase II as well. These subjects were expected to act helpless in the face of the outcome, rather than engaging in a strategy such as attempting to find the experimenter to determine what was the matter.

The results of the experiment were highly significant. Surprisingly, however, they were in the reverse direction of those predicted by the authors. A higher percentage of subjects in the "helpless" groups sought the experimenter's aid on the blurred trials than in the control groups. Subjects in the "helpless" groups stood up to get the experimenter significantly earlier than subjects in the control groups.

In addition to determining how subjects would react to this outcome, the investigators wished to determine subjects' proficiency at working the problems, as well as their reactions to several aspects of the experiment. At the end of Phase II, subjects who had participated in Phase I were asked to fill out a questionnaire that the experimenter had supposedly forgotten to administer. All subjects were then given a second questionnaire pertaining to Phase II. Responses to the first questionnaire revealed that the manipulation was successful. Subjects in the helpless groups reported feeling more like "no matter what" they couldn't solve the problems, less like their success or failure was under their control, more stressful, and more frustrated than subjects in the

contingent reinforcement control group. On responses obtained from the second questionnaire, subjects in the "helpless" groups reported feeling more like their success or failure was under their control than subjects in the control groups, and subjects in the "double helpless" group reported feeling more in control than subjects in the "helpless" group. A significant correlation was found between reported feelings of frustration and failure in Phase I and reported feelings of control in Phase II. There were no significant differences in problem-solving ability as evidenced by performance on the concept-formation task used in Phase II.

Roth and Bootzin's findings imply, then, that an individual who has had experience indicating that his efforts are independent of his outcomes will behave in a more assertive and less helpless manner than subjects who have not had such training. Of course, one can question Roth and Bootzin's operational definitions. One can certainly argue that "going and getting the experimenter" is a more helpless response than sticking it out and trying to complete the task on one's own. Unfortunately, Roth and Bootzin do not report whether the subjects who did not seek out the experimenter attempted to solve the problem on their own, or merely sat passively in front of the blurred stimulus. Perhaps their results reveal that those who have had experience with noncontingent reinforcement are quicker to seek others' aid than those who have not. However, this view of the results does not explain why subjects in the helpless groups reported feeling more like their success or failure was under their control

in Phase II than subjects in the control groups, and why subjects in the "double helpless" group reported greater feelings of control in Phase II than subjects in the helpless group.

Of course, it is important to understand the conditions under which loss of control leads to an aggressive response to regain control, and when it leads to depressed or helpless behavior. There are a number of interesting possibilities. One important variable may be the amount of experience one has indicating that he has no influence over his outcomes. Perhaps after a brief encounter with noncontingent reinforcement, individuals become strongly motivated to maintain control and exert themselves in an attempt to do so; but after repeated experiences with lack of control, they give up and become helpless. A second important variable may be the length of time elapsing since the uncontrollable negative outcome has occurred. Individuals may immediately react aggressively and hostilely, but over time become depressed and passive when they realize that their aggressive behaviors do not pay off. In line with this reasoning, Kubler-Ross (1969) has suggested that terminally ill patients initially become angry and hostile, but that in time the anger and rage are replaced by feelings of depression. A third variable of possible interest is whether there is any chance of gaining control over the aversive outcome that one has been exposed to. Glass and Singer (1972) have maintained that, if an individual believes that there is no possibility of gaining control over an aversive stimulus, he will become passive and compliant;

but if there is possibility for control, he will become aggressive and assertive. A clever experiment has been conducted by Shaban and Welling (reported in Glass & Singer, 1972) to test this assertion. These investigators were interested in how an individual would react if subjected to a frustrating bureaucratic experience. They reasoned that, if the experience were the result of the system itself, individuals would have no expectation of control, and would become helpless and compliant. If the experience were the result of the interaction with an unpleasant bureaucratic official, it was reasoned that the semblance of control--perhaps through persuasion and interaction--would seem possible. Individuals faced with this situation were expected to become aggressive and assertive in future interactions.

When subjects arrived for the session, they were told to report to an administrative assistant to complete some forms before participating. The administrative assistant gave subjects a questionnaire to complete. There were three experimental conditions. In the No Harassment control condition, all but a few questions had been crossed out and did not have to be answered by the subject. In the Personally Responsible condition, the assistant examined the form and informed the subject that it did not suit her and would have to be redone. The assistant also kept the subject waiting while talking to a friend on the telephone. In the Regulations Responsible condition, the assistant also requested that the subject redo his questionnaire. She made it clear that it was departmental regu-

lation, rather than her own desires, that required the subject to do this. She also kept the subject waiting while attending to a phone call, but her conversation revealed that the call was from a superior and concerned business matters.

After completing the form a second time and having it accepted, subjects returned to the first experimental room. They were then given a variation of Orne's (1962) "demand-characteristics" task. They were instructed to engage in behavior that was clearly useless, and the experimenter kept a record of whether they were compliant or whether they disobeyed the instructions they had been given. Subjects were then placed in a bargaining task with another subject, and their strategies were examined. The results strongly supported the authors' reasoning. Subjects in the Personally Responsible condition engaged in cheating on the Orne task, and adopted an aggressive bargaining strategy. Subjects in the Regulations Responsible condition were primarily compliant on the Orne task, and submissive and compliant on the bargaining task.

This experiment provides some evidence that an individual's expectations about control determine the way he reacts to an aversive outcome. However, the operational definition of control (the "Personally Responsible" versus the "Regulations Responsible" variable) is a bit broad, and one could certainly argue that many other variables have been manipulated simultaneously. Unfortunately, the investigators did not include questions designed to check the effectiveness of the manipulation,

and it is not clear whether subjects in the Personally Responsible condition in fact felt more potential control over the aversive exchange with the assistant than subjects in the Regulations Responsible condition.

Clearly, more research is necessary before we can determine how individuals respond when they have no control. But, taken as a whole, do these studies provide evidence that control is adaptive? As noted previously, the vast majority of experiments reviewed in this paper provide convincing evidence that the perception of control over a stressful stimulus reduces the number of aversive aftereffects and tends to result in lower autonomic reactivity during the administration of the stressful stimulus. Before concluding this section, it seems worthwhile to review a few sources of evidence suggesting that control is not adaptive. This possibility is certainly suggested by the results obtained by Roth and Bootzin (in press), since subjects in their experiment responded more helplessly in a problem-solving session when they had previously experienced control (i.e. contingent reinforcement) than when they had not. A second source of information suggesting that control is not adaptive comes from an observation by Richter (1957) concerning the behavior of wild and tamed rats. Richter has reported that wild rats, who presumably have developed the notion that there are contingencies between their behavior and their outcomes, immediately "give up" and drown when placed in a jar of water. In contrast, he reports that domesticated rats, who have presumably learned that there is some independence between their behavior

and their outcomes, continue swimming for several hours. Obviously, there are many other differences between wild and domesticated rats that could account for this finding, and no experiments that are conceptually similar to Richter's have been conducted with human subjects. More research is clearly needed to determine the conditions under which control in one situation leads the individual to believe that he can exert control in other situations as well, and the conditions under which evidence that one controls his outcomes causes an individual to "give up" at the first sign that this is not the case.

People attempt to control the uncontrollable

This section of the paper will focus on evidence that individuals make active attempts to control their environment. If individuals are indeed motivated to maintain effective control over their environment, it follows that they should feel threatened by outcomes that are typically viewed as uncontrollable--outcomes such as nature and chance, for example. This next section of the paper will summarize evidence--anecdotal as well as experimental--that individuals make active attempts to control such outcomes, and often delude themselves into thinking they have control over chance, fate, and luck.

How might an individual go about creating the illusion that he is in control of a chance outcome or a natural disaster? One possibility is to develop the belief that there is a contingency between his behavior and

the outcome--or between his behavior and the absence of the outcome. This type of behavior is often evidenced by gamblers, who engage in such tactics as talking to the dice and stereotyped throwing in order to influence their outcome. Janis (1951) reports that during the bombing raids in Britain, individuals developed hypotheses concerning the occurrence of the attacks. Such comments as "If we talk about it, it will happen" were very prevalent in the unbombed towns.

Both Janis (1951) and Kubler-Ross (1969) have pointed out that individuals often try to avoid negative outcomes by engaging in good behavior. According to Janis:

> People who are facing the prospect of illness, unemployment, or any extreme form of deprivation will often attempt to ward off the danger by making sure that they do not deserve to be punished. Evidently, this was one of the dominant types of reaction among the bombed population of Britain (pp. 169-170).

Similarly, Kubler-Ross (1969) has pointed out that terminally ill patients often engage in good and moralistic behaviors in an attempt to ward off death.

Individuals who have written about magic and rituals have indicated that these strategies are used in an attempt to exert control over phenomena that are not well understood. For example, Malinowski (1948) has mentioned that

> in lagoon fishing, where man can rely completely on his knowledge and skill, magic does not exist, while in the open-sea fishing, full of danger and uncertainty, there is extensive magical ritual to secure safety and good results (p. 31).

In addition to chance outcomes and outcomes brought about by nature,

there is another type of uncontrollable outcome that might be mentioned: an outcome that has already occurred. Freud (1920) observed that, after a traumatic experience of some type, it is typical for the victim to dream about the occurrence for days afterward. As a result of these observations, Freud added a new category of dreams--those motivated by a "belated attempt at mastery." This addition was a modification of his original theory in which he viewed all dreams as symbolic fulfillments of forbidden infantile wishes. Freud's observation is similar to one made by Goffman (1967) that when an individual elicits a negative reaction from someone in conversation, he has a strong desire to "drop through the floor" and "return magically to a point in time when it would have been possible to save face by avoiding the encounter (p. 16)."

Is there any experimental evidence that individuals are uncomfortable with the idea of a chance outcome, irrationally believe that they can control such outcomes, or make active attempts at controlling fate? Although no research has been directed toward answering these questions, a few studies designed for other purposes provide relevant information. A number of studies dealing with assignment of causality, for example, have described an outcome to the subjects, and have asked them to assign causality for the outcome to a variety of sources (see, for example, Streufert & Streufert, 1969; Walster, 1967). Studies of this type consistently find that subjects are very reluctant to attribute an outcome to chance. Subjects apparently prefer a situation in which someone or

something potentially controllable is responsible for people's outcomes.

There is also evidence suggesting that, under certain circumstances, people believe that they can control chance outcomes. For example, in an experiment by Badia, McBane, Suter, and Lewis (1966), subjects were shown two switches and were told that one would produce immediate shock and the other delayed shock. Subjects were explicitly told that "the total number of trials is fixed and your response will in no way affect how many shocks you receive (p. 848)." Nevertheless, Badia et al. report that, in the postexperimental interview, "several subjects adopted a 'gaming orientation' stating that the adoption of certain strategies minimized the intensity or reduced the number of shocks (p. 849)."

An interesting experiment by Strickland, Lewicke, and Katz (1966) also suggests that, under certain circumstances, individuals believe that they can influence chance outcomes. Subjects in this experiment were given some money by the experimenter and were told that they would be playing dice in this experiment. It was explained to the subject that he would make a bet and throw the dice 30 times. Subjects were led to believe that they could earn money if they won their bets. Strickland et al. were primarily interested in the level of conservatism or riskiness that would be exhibited by their subjects. They predicted that subjects who were asked to bet after they had thrown the dice would be more conservative than subjects who were asked to bet before throwing the dice. The independent variable, then, was the time at which the bet was made--

before or after the dice had been thrown.

The results were consistent with the authors' reasoning. The dependent variable calculated by the authors as an index of risk-taking behavior was the Maximum Possible Payoff--the amount the subject would have won if every bet he placed were successful. Subjects who bet before throwing the dice had a higher Maximum Possible Payoff than subjects who bet after the dice were thrown. The probability preference was also higher for the subjects who bet before the dice had been thrown. These subjects also tended to wager a greater total amount of money than subjects who bet after the dice had been thrown, but this difference fell short of significance ($p < .10$, one-tailed).

In an attempt to explain their findings, Strickland et al. (1966) have suggested that making the bet before throwing allows subjects to engage in such superstitious behaviors as stereotyped throwing, magical thinking, talking to the dice, etc. Their reasoning implies that the opportunity to engage in superstitious behaviors before the outcome occurs leads the subject to believe that he can influence the outcome. Unfortunately, Strickland et al. did not include a measure of perceived control, so it is impossible to know whether subjects who made their bet before throwing the dice believed that they had more control over the outcome than subjects who made their bet after the dice had been thrown.

That individuals make active attempts to influence fate is also suggested by the results of a dissonance experiment conducted by Allen (1964).

The purpose of this experiment was to resolve an ambiguity concerning an earlier study by Jecker (1964). Jecker (1964) had found that subjects do not show dissonance reduction if there is even a slight chance of receiving the rejected alternative along with the chosen one. It was not clear from Jecker's experiment whether the absence of dissonance reduction occurred because there was uncertainty involved with the outcome, or because of the possibility of obtaining the rejected alternative. In order to resolve this ambiguity, Allen's experiment varied both of these factors independently. Subjects were asked to rate several records, and were then given a choice between two they found moderately attractive. There were four experimental conditions: an "only chosen" condition, in which the subject was told that she would receive only the record she chose; a "nothing or chosen" condition in which the subjects were led to believe that they had a .50 probability of winning the chosen record and a .50 probability of winning nothing; a "chosen or both" condition, in which the subjects were told that they would definitely receive the chosen record, and had a .50 chance of receiving the rejected record as well; and a "nothing, chosen or both" condition, in which the subjects had equal chances of getting no records at all, of getting the chosen record, or of getting both records.

Subjects' responses faced Allen with a serious methodological problem: of the 121 subjects in the experiment, 24 showed decision reversals--they chose the record that they had initially rated as less attractive. The

interesting thing about these reversals is that they occurred in only two conditions of the study--the "nothing or chosen" condition and the "nothing, chosen or both" condition. In the other two conditions, subjects were assured of getting at least one record. For the subjects in these conditions, there was the possibility of getting nothing at all. Between one-third and one-half of the subjects who had the possibility of ending up with nothing showed decision reversals. In the other conditions, only about one in ten subjects showed a decision reversal. This difference was, of course, highly significant. Allen speculates that, when individuals are put in the hands of fate and have a chance of receiving a rather undesirable outcome, they may "feel that if they ask for less, then fate will be kind to them (p. 40)."

This anecdotal and experimental evidence suggests that individuals adopt a wide range of behaviors in order to maintain the belief that they are in control. The evidence cited supports the assertion of Geer, Davidson, and Gatchel (1970) that "perhaps the next best thing to being master of one's fate is being deluded into thinking he is (p. 738)." But because of the lack of experiments dealing directly with control, many ambiguities remain. The conditions under which individuals attribute control to themselves are far from clear.

Rationale for the current experiment

This experiment will attempt to provide answers for some of the many questions that have been raised about the concept of control. One

purpose of the study will be to obtain more information about the determinants of perceived control. Here, leads provided by previous studies will be followed up. Since almost all of the research cited has focused upon perceived control as an independent variable, we know relatively little about the factors that lead to perceived control. Because a few investigators have included manipulation checks, we do know that individuals perceive themselves to be in control when an instrumental response permits them to avoid the aversive stimulus, or when they are given "free choice" about pressing an escape button that will terminate the aversive stimulus. As noted previously, an interesting theoretical possibility concerning perceived control is provided by experiments that have manipulated who administers the stimulus. These studies suggest that merely causing a behavioral outcome for oneself may lead to the illusion of control. Unfortunately, these studies are so beset with methodological problems that it is impossible to draw conclusions from most of them. First, several of these experiments have confounded control and predictability by allowing subjects in the self-administered condition to predict as well as control the stimulus. Second, in at least one of these experiments, the duration of the aversive stimulus was not kept constant in the two experimental conditions. In the S-Caused condition, the subject merely turned the stimulus off, but in the E-Caused condition, the experimenter turned it off "with a delay equal to his reaction time." Third, although most of the investigators manipulating who administers

the stimulus have conceived of this variable in terms of control, not one has included a check on the manipulation by asking subjects if they indeed felt more control when they caused the stimulus than when the experimenter did. In the current experiment, an attempt will be made to examine the interesting theoretical possibility that merely causing an outcome for oneself leads to the perception of control, while avoiding the problems that have plagued previous studies.

In addition to causality, one other possible determinant of perceived control will be considered. The experiment cited previously by Strickland et al. (1966) suggests that causing an outcome is not enough to lead to the perception of control. All subjects in this experiment "caused" their outcome by throwing dice. Half of the subjects were asked to bet after they had thrown the dice; the remaining subjects were asked to bet before doing so. Subjects who bet after the dice had been thrown were quite conservative, suggesting that they did not believe they could control the outcome. But subjects who bet before the dice were thrown were found to be more risky in their strategy. This study suggests that individuals feel they are more in control if they know what they hope to obtain before causing the outcome than if they do not. Although this is an interesting possibility, Strickland et al. did not include a dependent measure on control, so it is impossible to tell whether subjects in one of the conditions felt more control over the outcome than subjects in the other. Taken as a whole, these studies suggest the hypothesis that, in order to

perceive control over a behavioral outcome, two factors are necessary: one must cause it, and one must know before causing it what one hopes to obtain. This hypothesis will be tested in the current experiment.

Although the results of many experiments suggest that control is adaptive, few provide insights into why this may be so. A second purpose of this experiment is to explore some variables that may mediate the effects of perceived control. One possibility is that subjects who believe that they have control over an outcome have greater feelings of choice and responsibility for the outcome. As mentioned earlier, most of the studies on perceived control have not even included a check on the control manipulation. None of these experiments has asked subjects about feelings of choice or responsibility.

A third purpose of the current experiment is to determine the effects of perceived control on outcome evaluation. Fifteen years of experiments stemming from dissonance theory have suggested that, if an individual chooses an outcome with negative consequences and feels responsible for it, he will come to view it more positively. If an individual controls an outcome, will a similar effect occur? Particularly if perceived control leads to feelings of choice and responsibility, we would expect individuals to view a controlled outcome more positively than an uncontrolled one.

A final purpose of this experiment is to learn more about how individuals respond to uncontrolled outcomes. Past research has exposed

subjects to uncontrollable outcomes (cf. Seligman's uncontrollable shock) and then examined their reactions in a different setting. Few investigators have focused on the response to the uncontrolled outcome itself. Although dissonance theory and reactance theory generate predictions concerning responses to outcomes that the individual chooses, they provide no insight into the response to an outcome that occurs by chance, and occurs independently of the individual's behavior. The present experiment was designed to permit the observation of changes in evaluation that occur once an individual receives an uncontrolled outcome.

In order to ascertain the importance of causality, the following variable was decided upon: the outcome that some subjects received was determined by the color of a marble they obtained by reaching into a can containing two marbles of different colors; the outcome that others received was determined by the color of a marble picked by the experimenter. In both cases, the marbles were placed into the can and mixed up, and the person picking the marble could not see into the can. This may be regarded as a rather strict test of the hypothesis that causing an outcome leads to feelings of control, since in all cases the outcome was obviously determined by chance. Hopefully, this manipulation avoids confounding causality of the outcome with such factors as predictability or the length of time that the outcome is experienced. In order to ascertain the importance of foreknowledge (i.e. knowing before the outcome occurs what one hopes to obtain), some subjects were told before the marble was

picked which color of marble stood for which outcome, and some were not told this until after their marble had been picked. Questions concerning perceived control, choice, and responsibility were included as dependent measures.

In order to check on the possibility that reactions to controlled or uncontrolled outcomes may depend upon the outcome's attractiveness, attractiveness was included as a variable. In departure from previous studies, however, only positive outcomes were used. The outcomes employed in the study were consumer items. Half of the subjects received an item that they found very attractive; the remaining subjects received an outcome that they found minimally attractive. The decision to use positive outcomes was made because of a possible ambiguity in the use of negative ones. It was felt that the use of negative outcomes may lead subjects to feel that they had been taken advantage of, or treated unfairly. There are two reasons why an attempt was made to avoid this feeling on the part of the subjects. First, it was believed that such feelings might preoccupy subjects to a point where the other experimental variables would create less impact. Second, it is possible that such feelings may interact with the control variable to produce artifactual differences in subjects' responses. Glass and Singer (1972) mention the possibility that an experimenter who allows control over a negative outcome may be regarded differently than one who does not allow control, and that subjects who have control may view the experiment as more fair than

subjects who do not. Glass and Singer checked on this possibility in only one of their many experiments on control (see pp. 74-78). Although subjects did not view the experimenter differently in this particular experiment, they did tend to view the entire setup as less fair when they did not have access to an escape button than when they did. Glass and Singer found that subjects without control did less well on a proofreading task than subjects with control. They argued from these data that lack of control over an aversive stimulus impedes later performance. An alternative explanation is that subjects who did not have control were angry that they had been treated unfairly, and did not try as hard to do well on the proofreading task as the subjects who did have control. In the current experiment, positive outcomes were used in order to avoid this problem. An attempt was made to design the experiment so that the fate each subject received would undoubtedly be a fair one. All subjects were promised remuneration for their participation ($2.50 for about 40 minutes), and received a very nice or a minimally attractive consumer item in addition to the money they had been promised.

In order to determine the effects of perceived control on outcome evaluation and the reactions to uncontrolled outcomes, subjects were asked to rate the consumer items before they found out that any would be obtained, and again after they had received one of the items.

The experiment itself contained the following conditions: an E-Caused Foreknowledge condition, in which the experimenter picked a

marble and assigned the subject an outcome, and the subject knew beforehand which marble stood for which outcome; a S-Caused Foreknowledge condition, in which the subject picked a marble and assigned himself an outcome, and knew beforehand which marble stood for which outcome; and a S-Caused No Foreknowledge condition, in which the subject picked a marble and assigned himself an outcome, but was not told until after he picked the marble which color stood for which outcome. Cross-cutting these conditions was the attractiveness variable: half of the subjects received the marble which resulted in their winning a highly attractive item; the remaining subjects received the marble which resulted in their winning a minimally attractive outcome. If the hypothesis suggested earlier is correct, and both causality and foreknowledge are necessary for perceived control, the S-Caused Foreknowledge subjects should perceive themselves to have more control than subjects in the other two conditions, and subjects in the other two conditions should not differ from one another in feelings of perceived control. No predictions were made concerning the attractiveness variable, or interactions between the two variables.

It was decided that an E-Caused No Foreknowledge condition was not essential for the design. Extensive pretesting had revealed that subjects in an E-Caused Foreknowledge condition experienced no feelings of control whatsoever. Therefore, it was thought to be an impossibility that subjects in an E-Caused No Foreknowledge condition would experience

significantly less control than these subjects. It was also thought to be extremely unlikely that subjects in an E-Caused No Foreknowledge condition would exhibit more perceived control than subjects in the E-Caused Foreknowledge condition, since the only study dealing with this variable has suggested that subjects with foreknowledge attribute more control to themselves than subjects without it.

METHOD

Overview

Subjects had signed up for an experiment on young people's attitudes and values, and were first asked to rate eight consumer items according to how much they liked each one. The experimenter then explained that, since two of the manufacturers had given her a large number of the items, she was going to give an item to each student who participated. Each subject was shown two items--one that he had previously rated high, and one he had rated low, and was told that these were the items that had been donated. Subjects were told that, since it was necessary to give away equal numbers of the items, the item each person received would be determined by chance--by the color of a marble pulled from a coffee can. Each subject was asked to take a red and a blue marble from a dish of marbles and hand them to the experimenter, who placed them in an empty coffee can. One-third of the subjects were told that the experimenter would pick out a marble, and were told which color of marble stood for each prize. One-third were told that they should pick out a marble, and were told before picking which color of marble stood for each prize. The remaining subjects were told that they should pick out a marble, but were

told that after they picked the marble they would find out which color stood for each prize. Half of the subjects picked the marble that led them to win the highly attractive prize; the remaining subjects received a marble assigning them to the less attractive prize. After a few minutes, subjects were asked to complete questionnaires concerning their perceptions of control, their feelings of choice, responsibility, and fairness, as well as their opinion of the experimenter, and the experiment itself.

Subjects

The subjects were 65 male undergraduates from the Lake Forest College, in Lake Forest, Illinois.[1] Subjects were recruited by means of posters offering $2.50 for their participation in the study on young people's attitudes and values. None of the subjects had previously participated in psychological research, and the majority of the students had no background in psychology. Subjects were tested in groups of three; each

---

[1]Of the 65 subjects who participated in the experiment, 58 were included in the analyses. Of the remaining subjects, one was discarded because he rated all of the consumer items below the midpoint on the rating scale, and one was excluded because he indicated that he did not want either of the consumer items that were available. In addition, 5 subjects were excluded because they believed that their initial ratings of the items had influenced the items that the experimenter said were available. These subjects were in the following experimental conditions: 3, S-Caused Foreknowledge High Attractive; 1, S-Caused No Foreknowledge High Attractive; 1, S-Caused No Foreknowledge Low Attractive; 1, E-Caused Foreknowledge High Attractive; and 1, E-Caused Foreknowledge Low Attractive. The inclusion of these subjects would in no way have altered the results reported or the conclusions drawn.

experimental session lasted approximately one hour.

Procedure

When the subjects arrived, they were reminded that the experiment concerned young people's attitudes and values. They were told that the experimenter would be asking them many questions about their opinions. It was explained that their responses would be used to study a variety of problems, including how values change and cross-cultural differences in values. Subjects were told that their responses would be compared to those made by college students in past years, as well as those of college students in different countries. The subjects were forewarned that there would be a large number of questionnaires in the study, but were told that the experimenter would try to break these up so that they did not become too tiring. They were instructed to be as accurate and honest about their feelings as possible when completing the questionnaires.

The experimenter told the subjects that she first wanted to get some information concerning the kinds of consumer items that are attractive to young people today. They were told that, in a few moments, they would be taken to a booth containing eight consumer items, and that they would be asked to examine these items very carefully. Subjects were told that they would be given about 5 minutes to examine these items, and that they should think about what they would do with each one if they owned it, how much they liked it, how much they would be willing to pay for it, etc.

They were told that, after they had a chance to examine each of the items, they would be asked to rate them for us.

At this point, each subject was taken to a separate booth. Each subject saw eight items in front of him; four had been selected to be very attractive, and four had been selected to be minimally attractive. The four attractive items included a large, scented candle, a handmade pottery coffee mug, an official English dartboard game, and a set of six Scotch glasses. The four minimally attractive items included a scratch pad, a back scratcher, a small plastic pillow, and a child's pea shooter game. The average value of the former items was $3.00; the average value of the latter items was 20 cents. Subjects were given five minutes to inspect the items in any order they wished. This rather long inspection period was included to minimize the problems with unreliable ratings.

After examining the items, subjects were given a rating scale (to be described later), and were asked to rate each item according to how much they liked it. They were also asked to indicate whether they owned any of these items, and, if so, if the items they owned were similar to those presented here. They were told to try to be as discriminating as possible, and to avoid giving two items the same rating unless it was absolutely necessary. They were also asked to be extremely careful in making their ratings, and to try and decide how they "really feel" about each item before rating it.

After a few moments the experimenter collected their ratings, and

explained that she would next like the subjects to judge some current advertisements. Each subject was given a recent issue of Life magazine and a questionnaire. Subjects were instructed to rate eight advertisements from the magazine. They were told that their ratings of these advertisements would provide useful information about their reactions to current products. In reality, this was a filler task. While the subjects were working on these ratings, the experimenter examined the consumer item ratings that they had made previously. For each subject, two consumer items were selected to serve as the outcomes in the experiment. The experimenter selected one item for each subject that was rated high, but at least three points from the top of the scale, and one that was rated low, but at least three points from the bottom of the scale. Items rated at or very near the end of the scale were avoided since such items would have made it difficult to detect changes in attractiveness. Of course, an item was not picked to serve as an outcome for a particular student if he indicated that he already owned it. Using this procedure, most students received an opportunity to win the item that they had rated second highest and seventh highest.

After the experimenter had finished this task, she collected the subjects' questionnaires concerning current advertisements. She then pointed out that she realized that answering so many questionnaires can get tiring, and that she was going to do something now to break up the questionnaires--something that she would not normally do until the end of

the study. She explained that when she went to get the items for the experiment, most of the manufacturers insisted on charging for them. She pointed out that two of the manufacturers were very happy that their products were going to be used in this study because of the free publicity, and had therefore donated a large sample of their items. The experimenter explained that, since the manufacturers had been kind enough to provide such a large supply of these items, she was going to give one to each student who participated in addition to the $2.50. She mentioned, however, that, since she had to give away equal numbers of the items, it would not be possible for students to choose which item they wanted:

> In a moment I'll come around to each booth and show you the two items that are available. Which of these items you get will be determined by chance--by the color of a marble that is drawn from a coffee can. This way, everyone will have a fair chance of getting the item that he prefers.

The subjects' attention was then directed to a dish of red and blue marbles, a red and blue mat, and an empty coffee can that had been placed in a corner of each booth. All subjects were asked to pick a red and a blue marble out of the dish and hand them to the experimenter when she came by. They were also asked to check the coffee can to make sure nothing was in it. The experimenter then came around to each booth and showed each subject which two items had been "donated." She placed an item that the subject had rated high on one of the colored mats, and an item that the subject had rated low on the other colored mat.

At this point, the experimenter turned over a card informing her

of the experimental condition, and the instructions differed depending upon the condition to which the subjects had been assigned. In the Experimenter-Caused Foreknowledge condition, the subjects were told:

> I will put the two marbles that you hand me in the coffee can, mix them up, reach in without looking and pull one out. If I pull out the red marble, you will get the item that is on top of the red mat. If I pull out the blue marble, you will get the item that is on the blue mat.

In the Subject-Caused Foreknowledge condition, the experimenter said:

> I would like you to put the two marbles in the coffee can, mix them up, reach in without looking and pull one out. If you pull out the red marble, you will get the item that is on top of the red mat. If you pull out the blue marble, you will get the item that is on the blue mat.

In the Subject-Caused No Foreknowledge condition, subjects were told:

> I would like you to put the two marbles in the coffee can, mix them up, reach in without looking, and pull one out. After you pull one out, you can turn over the two small cards I will put on your desk. The cards will tell you which item you will get if you pulled out the red marble, and which item you will get if you pulled out the blue one.[1]

The purpose of letting the subjects select the two critical marbles from a large dish of marbles was to minimize suspicion in the E-Caused condition. It was felt that, if the experimenter selected the marbles, subjects

---

[1] The purpose of using these cards rather than having the experimenter simply tell the subject what item his marble stood for was to minimize suspicion. It was felt that problems with suspicion would be especially likely in the low attractive outcome condition. Suppose the experimenter were to tell the subject after he had picked out the red marble: "OK, that means you get the back scratcher instead of the candle." Subjects might conclude that the experimenter never intended to give them the candle, and would have told them that they had won the back scratcher regardless of which marble they picked. By picking a red marble and then turning over cards that say, "Red = Back Scratcher" and "Blue = Candle," a subject can see that this is clearly not the case.

might think that they differed in size or texture and that the outcome was rigged. Marble colors were counterbalanced so that, half the time, getting the red marble meant getting the highly attractive item, while the remainder of the time getting the blue marble meant getting the highly attractive item.

After explaining how the marble was to be picked, the experimenter went around to each booth so that each subject could pick or receive a marble, and then said to him: "OK, that means you get this item." Prior to receiving their marble, subjects were instructed to keep their reactions to the prize "to themselves," so as not to "bias the other subjects' opinions." Half of the subjects received a marble assigning them to the attractive prize; the remaining subjects received a marble assigning them to the minimally attractive prize.

After each subject's outcome had been determined, a second "filler" task was instituted. It was reasoned that a time delay before reevaluation of the outcomes might be desirable (cf. Walster, 1964). Therefore, subjects were given a task that took about five minutes, but that would permit them to think about the items they had obtained if they wished to do so. It was explained to the subjects that, in addition to obtaining information about their attitudes and values, "we would also like to obtain some information about the kinds of things young people typically think about." Subjects were told:

> In order to investigate this question, we are going to try something called "free association." For the next five minutes, I would like you

> to sit and just relax. During this time, several things may come into your mind. You may think about one of the courses you are taking. Or you may have thoughts about a girl or boyfriend. Or you may think about something that has happened to you recently, or about some aspect of this study. Whenever some thought comes into your mind, please write it down on the blank card I am going to give you, no matter how silly or trivial that thought might seem to you.

While the subjects worked on this task, the experimenter went to the supply closet and got the items that each subject had won. She placed the item that had been won next to each subject so that he would not have doubts about actually receiving the item.

After five minutes had passed, the experimenter collected the cards and informed the subjects that she would like them to rate the consumer items again. The subjects were handed a rating sheet, along with a title sheet that provided a rationale for a second rating. This sheet informed the subjects that "we are interested in how people's opinions of consumer items change as they become familiar with the items." It was pointed out that some investigators feel that individuals become more favorable in their attitudes, while others believe that individuals become negative. The sheet instructed subjects to take a few moments to reexamine the items, and to rate them according to how they felt about them "now."

After completing these ratings, subjects were told:

> Now I would like you to think back on part of the experiment where I put two items out in front of you and told you that you would be receiving one of these items. We're interested in what your feelings were during this time, and the next questionnaire asks you some questions about your reactions to this part of the study.

This questionnaire contained questions designed to check the effectiveness

of the experimental manipulations, and to ascertain subjects' feelings of control, choice, and responsibility. Subjects were also asked whether they perceived the outcome of the experiment as fair.

After completing this questionnaire, one final request was made of the subjects. They were told that the individuals who were funding the research were interested in how the experimenters were "coming across" to the students. Subjects were given an anonymous questionnaire that asked them to evaluate the experimenter and the experiment. They were instructed to place their completed questionnaire in a sealed envelope and to place it in a box in front of the room. It was stressed that they should be as honest as possible in their reactions, and that the experimenter herself would never see the ratings. The main reasons for including this final questionnaire were: (a) to ascertain whether subjects' opinions of the experimenter were affected by the experimental manipulations; and (b) to determine whether the subjects thought the outcome was "fixed" in any of the conditions. As noted previously, many precautions were taken so that the subjects would not regard the E-Caused outcome as fixed. Such a hypothesis on the part of the subjects would provide a rather trivial explanation for any differences in ratings in the E-Caused and S-Caused conditions.

After this final questionnaire had been collected, a postexperimental interview was conducted in which the experimenter probed for suspicion. Subjects were given additional details about the actual purpose of the

experiment, and were encouraged to ask as many questions as they wished about the experiment. They were permitted to keep the consumer item they had won, although they were given a receipt and asked to pick it up after all the subjects had been run. At the end of the interview, subjects were paid the $2.50 they had been promised, and were asked not to discuss the experiment with other potential subjects.

Dependent measures

All of the dependent measures administered in the experiment have been reprinted in Appendix II. Subjects were asked to indicate how much they liked each consumer item on a 20-point scale marked with the following labels: "not at all," "slightly," "moderately," "pretty much," and "very much." These ratings were made once at the beginning of the experiment, and once after the subject had received his item. Differences between the two ratings were taken as a measure of change in item attractiveness.

On the questionnaire administered immediately after the subject had rerated the items, he was asked to indicate which item he hoped he would get. This question was included as a check on the attractiveness manipulation. Subjects were also asked to estimate their subjective probability of getting the item they hoped for, and were asked to indicate which item they expected to get. Then, on a series of 20-point scales, subjects were asked about perceived control ("How much did you feel that you could influence what color of marble you received?"), choice ("How much

choice do you feel you had about which item you got?"), and responsibility ("How responsible would you say you are for the fact that you got this item instead of the other one?"). Subjects were also asked two questions concerning their perceptions of the fairness of the procedure. On 20-point scales, they were asked to indicate "How fair do you think it is that you got this item instead of the other one?" and "In general, how fair would you say the procedure is for deciding who gets which item?"

Following this questionnaire, subjects received an "anonymous" questionnaire designed to elicit their reactions to the experiment and the experimenter. The main purpose of this "anonymous" questionnaire was to determine whether subjects thought the outcome had been "fixed." As noted previously, rather elaborate precautions were taken to avoid this conclusion on the part of the subjects, since this feeling could lead to a rather trivial explanation for any differences between the E-Caused and S-Caused conditions. It was thought that asking subjects this question directly might plant some suspicions that were not already there. Therefore, subjects were first asked a general open-ended question concerning the portion of the experiment when the marble was selected. They were asked to indicate whether there was anything about this part of the study that bothered them. They were also asked whether the experimenter had done or said anything during this part of the study that annoyed them. Then, on the following page of the questionnaire, they were asked a specific question on this subject. On a scale ranging from 0 ("No, not at all")

to 20 ("Very much so"), they were asked to indicate whether they ever had the feeling that the outcome was fixed--that the experimenter had somehow cheated to make sure that they got one item instead of the other.

Subjects were also asked to indicate how much they liked the experimenter. In addition to these questions, a number of filler items were included on the questionnaire. Subjects were asked to rate the experimenter on a variety of dimensions, such as her proficiency in passing out materials and her clarity in describing the purpose of the experiment. The purpose of these filler items, of course, was to lend credibility to the rationale provided for this questionnaire.

RESULTS

Check on the effectiveness of the manipulations

There is no doubt from the subjects' ratings that the attractiveness manipulation was effective. The mean for the high attractive item was 15.5; the mean for the low attractive item was 3.81, and there was no overlap between the distributions. As an additional check on the attractiveness manipulation, subjects were asked to indicate which item they hoped they would get. All but one subject said that they hoped they would get the item that they initially rated as more attractive. Although there was no check on the causality and foreknowledge manipulations, the results leave little doubt that these manipulations were effective.

Major analyses

In order to evaluate the effect of the treatments, and to provide a test for the assertion that two attributes--causality and foreknowledge--are necessary for perceived control, the following orthogonal comparisons were performed:[1] (a) Number of Attributes (the S-Caused Foreknowledge

[1]With this type of design, a number of different analyses are possible. One possibility is to conduct two separate 2 x 2 analyses of variance--one using the E-Caused Foreknowledge and the S-Caused Fore-

subjects versus the subjects in the other conditions); (b) Attributes (the E-Caused Foreknowledge subjects versus the S-Caused No Foreknowledge subjects); (c) Attractiveness (subjects receiving the high attractive item versus subjects receiving the low attractive item); (d) the Number of Attributes x Attractiveness interaction; and (e) the Attributes x Attractiveness interaction.

Perceived control

Subjects' mean ratings of perceived control are presented in Table 1. The analyses provide strong support for the hypothesis that both causality and foreknowledge are necessary for feelings of control. Subjects in the S-Caused Foreknowledge condition felt they could influence what color of marble they received significantly more than subjects in the other two conditions ($F = 4.815$; $df = 1, 52$; $p < .033$). Subjects in the E-Caused

---

knowledge cells (E-Caused versus S-Caused x High versus Low Attractiveness), and one using the S-Caused Foreknowledge and the S-Caused No Foreknowledge cells (Foreknowledge versus No Foreknowledge x High versus Low Attractiveness). Another possibility is to conduct a 3 x 2 analysis of variance (E-Caused Foreknowledge versus S-Caused Foreknowledge versus S-Caused No Foreknowledge x High versus Low Attractiveness), and, using the overall error term, conduct a series of comparisons between various cells. It should be pointed out that the conclusions reported in the paper would not change had the data been analyzed by one of these alternative methods. It should also be added that a multivariate analysis, in which all of the dependent measures were combined to yield a single F value, was conducted on the data. In order to be as conservative as possible, even the filler items on the "anonymous" questionnaire (see Appendix II) were included. This analysis revealed a highly significant effect for Number of Attributes (S-Caused Foreknowledge subjects versus E-Caused Foreknowledge and S-Caused No Foreknowledge subjects; Multivariate $F = 2.906$, $p < .004$).

Table 1

Means and Standard Deviations for the Perceived Control Question

Attractiveness		Causality and Foreknowledge		
		E-Caused Foreknowledge	S-Caused Foreknowledge	S-Caused No Foreknowledge
Low	M	1.70[a]	6.50	.70
	(SD)	(5.39)	(8.08)	(2.21)
	N	N = 10	N = 10	N = 10
High	M	2.00	4.80	4.25
	(SD)	(6.32)	(5.09)	(6.69)
	N	N = 10	N = 10	N = 8

[a] 0 = "not at all"; 20 = "very much."

Foreknowledge condition and the S-Caused No Foreknowledge condition did not differ in their feelings of control ($F < 1$). Neither the main effect for attractiveness nor the interactions revealed any significant differences between conditions.

Choice and responsibility

Subjects' ratings of choice and responsibility are presented in Tables 2 and 3. It can be seen that these ratings closely parallel subjects' ratings of control. A comparison of the S-Caused Foreknowledge condition with the other two conditions revealed a strong effect for both choice ($F = 42.869$; $df = 1, 52$; $p < .001$) and responsibility ($F = 17.130$; $df = 1, 52$; $p < .001$). Again, there were no significant differences between the E-Caused Foreknowledge condition and the S-Caused No Foreknowledge condition (both F's < 1), suggesting that causality or foreknowledge alone do not affect perceived choice or responsibility. No significant differences were obtained for the attractiveness variable, or the interaction between the variables.

An examination of the standard deviations presented in Tables 1, 2, and 3, as well as the raw data presented in Appendix I, reveals that there is quite a bit of within-cell variability in subjects' perceptions of control, choice, and responsibility. Most of the subjects responded to these questions by perceiving either no control, choice, or responsibility at all or by perceiving themselves to have a great deal of control, choice, and

Table 2

Means and Standard Deviations for the Perceived Choice Question

Attractiveness		Causality and Foreknowledge		
		E-Caused Foreknowledge	S-Caused Foreknowledge	S-Caused No Foreknowledge
Low	M	.30[a]	8.20	0.00
	(SD)	(.95)	(7.58)	(0.00)
High	M	0.00	7.30	2.88
	(SD)	(0.00)	(3.56)	(4.52)

[a]0 = "no choice at all"; 20 = "complete freedom of choice."

Table 3

Means and Standard Deviations for the Perceived Responsibility Question

Attractiveness		Causality and Foreknowledge		
		E-Caused Foreknowledge	S-Caused Foreknowledge	S-Caused No Foreknowledge
Low	M	2.00[a]	8.80	1.00
	(SD)	(4.83)	(9.44)	(3.16)
High	M	0.00	6.50	3.13
	(SD)	0.00	(4.86)	(5.52)

[a] 0 = "not at all responsible"; 20 = "completely responsible."

responsibility. Therefore, the data deviate somewhat from a normal distribution. Although analysis of variance is regarded as relatively robust under conditions of nonnormality, nonparametric tests were performed on the data as a precautionary measure. Subjects were divided at the median according to their feelings of control, choice, and responsibility, and these frequencies are presented in Table 4. Chi-square tests were performed on the frequency data. A comparison of the S-Caused Foreknowledge condition with the remaining conditions again revealed highly significant effects for control ($X^2 = 12.258$; df = 1; $p < .001$), choice ($X^2 = 28.323$; df = 1; $p < .001$), and responsibility ($X^2 = 14.730$; df = 1; $p < .001$). No significant differences emerged from a comparison of the E-Caused Foreknowledge and the S-Caused No Foreknowledge conditions, or a comparison of the High versus Low Attractiveness conditions.[1]

Outcome evaluation data

As noted previously, subjects were asked to rate each item upon entering the experiment, and again after receiving one of the items. This made it possible to compute changes in attractiveness for both the obtained item and the nonobtained item. These changes in attractiveness are

[1]This particular partitioning of the chi-square contingency table is recommended by Bresnahan and Shapiro (1966); if the E-Caused Foreknowledge cells and the S-Caused No Foreknowledge cells are listed in separate rows, the chi-square values for control, choice, and responsibility are still highly significant. Furthermore, highly significant chi-square values are obtained for these measures if the S-Caused Foreknowledge cells are compared with either the E-Caused Foreknowledge cells or the S-Caused No Foreknowledge cells.

Table 4

Frequencies with Which Subjects Felt High or Low Control, Choice, and Responsibility

	Low (0)	High (> 0)
Control		
S-Caused Foreknowledge	7	13
E-Caused Foreknowledge	18	2
S-Caused No Foreknowledge	14	4
Choice		
S-Caused Foreknowledge	3	17
E-Caused Foreknowledge	19	1
S-Caused No Foreknowledge	15	3
Responsibility		
S-Caused Foreknowledge	6	14
E-Caused Foreknowledge	18	2
S-Caused No Foreknowledge	14	4

presented in Table 5. In order to provide an overall measure of outcome evaluation, each subject's change in attractiveness for the nonobtained item was subtracted from his change in attractiveness for the obtained item. This overall figure of outcome evaluation is also presented in Table 5; the higher the mean, the greater the subjects' tendency to enhance the obtained item relative to the nonobtained one.

Since it has been demonstrated that causality and foreknowledge induce feelings of control, choice, and responsibility, it is reasonable to assume that subjects in the S-Caused Foreknowledge condition will come to view their obtained outcome more favorably relative to the nonobtained outcome. The results of each orthogonal contrast are presented in Table 6. These results reveal that there are no effects of any kind on the outcome evaluation measures. Causality and foreknowledge do not affect changes in evaluation. Subjects do not respond differently to a highly attractive outcome than to a minimally attractive one, and there are no interactions between the variables.

An examination of the cell means in Table 5 suggests that subjects tend to derogate the obtained item relative to the nonobtained one in two cells: the E-Caused Foreknowledge Low Attractiveness condition, and the S-Caused Foreknowledge High Attractiveness condition. If subjects who receive a low attractive outcome are considered separately, a comparison of the E-Caused Foreknowledge subjects with the other two groups reveals that they show a marginally significant tendency to derogate the

Table 5

Changes in Evaluation of Obtained and Nonobtained Outcomes

Attractiveness	Causality and Foreknowledge		
	E-Caused Foreknowledge	S-Caused Foreknowledge	S-Caused No Foreknowledge
Low			
Obtained	-1.70[a]	0.00	.90
Nonobtained	.10	.90	- .50
Difference	-1.80	- .90	1.40
High			
Obtained	- .90	-1.40	- .25
Nonobtained	- .10	.90	.75
Difference	- .80	-2.30	-1.00

[a]Positive scores indicate increased attractiveness; negative, decreased.

Table 6

Orthogonal Contrasts for Changes in Attractiveness of the Obtained Item, the Nonobtained Item, and the Difference Between the Two

Source	df	MS	F	p
Obtained Item				
No. of attributes (A_1)	1	.685	< 1	
Attributes (A_2)	1	25.981	2.439	.124
Attractiveness (B)	1	4.555	< 1	
A_1 X B	1	4.900	< 1	
A_2 X B	1	8.950	< 1	
Nonobtained Item				
No. of attributes (A_1)	1	9.753	1.308	.258
Attributes (A_2)	1	.050	< 1	
Attractiveness (B)	1	1.437	< 1	
A_1 X B	1	.900	< 1	
A_2 X B	1	4.950	< 1	
Difference Between Obtained and Nonobtained				
No. of attributes (A_1)	1	9.753	< 1	
Attributes (A_2)	1	23.740	1.408	.241
Attractiveness (B)	1	11.108	< 1	
A_1 X B	1	1.600	< 1	
A_2 X B	1	27.200	1.614	.210

obtained outcome ($F = 2.894$; $df = 1, 52$; $p < .095$). However, if subjects who receive a high attractive outcome are considered separately, there is no significant difference between the responses of the S-Caused Foreknowledge subjects and those of the other subjects.[1]

Perhaps the most surprising thing about the outcome evaluation data is that, although subjects in the S-Caused Foreknowledge cells manifest feelings of choice and responsibility, they do not show a "dissonance effect." In fact, they show a slight tendency to derogate the obtained outcome relative to the nonobtained one. One may argue that, although these subjects felt more choice and responsibility than subjects in the other conditions, the amount of choice and responsibility was not sufficient to arouse dissonance. However, if the subjects manifesting a high degree of choice or responsibility are considered separately, they do not show a "dissonance effect" either. In fact, the correlations between the subjects' ratings of perceived control, choice, and responsibility and their evaluation of the outcomes are minimal and do not even approach significance. Interestingly, the control, choice, and responsibility ratings are intercorrelated with one another. Perceived control ratings show a correlation of .48 with perceived choice ratings and of .35 with perceived responsibility ratings; perceived choice and responsibility ratings are correlated

---

[1]However, for the overall outcome evaluation measure, the mean in the S-Caused Foreknowledge High Attractive cell was found to be marginally different from zero change ($F = 3.318$; $df = 1, 52$; $p < .082$). None of the other means approached significance when compared to zero change.

at the .61 level. All of these correlations are significant beyond the .05 level. The failure to obtain any significant changes in outcome evaluation for controlled or uncontrolled outcomes, or any relationship between control, choice, and responsibility ratings and outcome evaluation will be considered more fully in the discussion section.

Additional measures

On the questionnaire dealing with control, responsibility, and choice, subjects were also asked to state their subjective probability about winning, to indicate which item they expected to win, and to indicate whether they felt the procedure was fair. The means and standard deviations for subjects' subjective probability estimates are reported in Table 7. All of these means are quite close to the actual probability of .50, and none of them differ significantly from one another. This suggests that the majority of subjects in the experiment perceived the probability accurately.

When asked which item they expected to win, subjects could indicate that they expected to win the attractive item or the unattractive item, or could avoid making an expectation. Subjects' expectations are presented in Table 8. A chi-square analysis performed on these frequency data revealed that subjects who won the high attractive item indicated that they expected to do so, while subjects who won the low attractive item indicated that they expected to do so ($X^2 = 6.81$; $df = 2$; $p < .05$). This

Table 7

Means and Standard Deviations for Subjects' Estimates of Their Chances of Getting the Item They Wanted Most

Attractiveness		Causality and Foreknowledge		
		E-Caused Foreknowledge	S-Caused Foreknowledge	S-Caused No Foreknowledge
Low	M	50.00	49.70	47.30
	(SD)	(4.71)	(26.73)	(5.93)
High	M	55.40	48.00	56.25
	(SD)	(18.66)	(7.13)	(11.57)

Table 8

Subjects' Expectations Concerning Which Item They Would Win

Expectations	Causality and Foreknowledge								
	E-Caused Foreknowledge			S-Caused Foreknowledge			S-Caused No Foreknowledge		
	Low	None	High	Low	None	High	Low	None	High
Attractiveness									
Low	7	3	0	4	1	5	6	4	0
High	1	4	5	5	1	4	2	2	4

provides corroboration for Walster's (1967) hypothesis that, after an event has occurred, individuals exaggerate the extent to which they would have predicted it. If one examines the S-Caused Foreknowledge subjects separately, however, one notices that this pattern does not occur for these subjects. About half of the subjects in this condition indicated that they expected to get the high attractive item; the remaining subjects indicated that they expected to get the low attractive item. One possible interpretation of this finding is that, in contrast to the other conditions, the S-Caused Foreknowledge condition induced very strong expectations in the subjects. Perhaps when confronted with a potentially controllable situation, some individuals react with feelings that they are definitely going to get what they want, while others feel that they are definitely going to get something undesirable. Perhaps these expectations are so strong that they are not influenced by the outcome when it occurs. It is possible that, in the other situations, subjects saw no potential for control and therefore did not develop strong expectations about what they would win. When asked after receiving an outcome to indicate what outcome they expected, they may then have let their outcome influence their feelings about their expectations.

Subjects were also asked two questions about the fairness of the procedure. First, they were asked how fair it was that they got the item they received instead of the other one. Second, they were asked how fair the procedure is for deciding who gets which item "in general." Means

and standard deviations for both questions are reported in Table 9. The means for the first question were not found to be significantly different from one another. However, analyses performed on the second question revealed a main effect for outcome. Subjects who received a highly attractive outcome perceived the procedure to be significantly more fair than subjects who received a minimally attractive outcome ($F = 5.967$; $df = 1, 52$; $p < .018$). Additional contrasts revealed that this difference was due primarily to subjects in the E-Caused Foreknowledge condition. When these subjects are considered separately, subjects who receive a low attractive outcome view the procedure as significantly less fair than subjects who receive a high attractive outcome ($F = 3.899$; $df = 1, 52$; $p < .054$). The effect for outcome is not reliable when either of the other groups is considered separately.

On an "anonymous" questionnaire completed next, subjects were asked to indicate whether they thought the outcome was "fixed," and were asked to give their opinion of the experimenter. On an open-ended question, subjects were asked to think back to the part of the experiment where the marble was drawn. They were asked to indicate if anything about that part of the experiment had bothered them, or if the experimenter had done anything during that time that annoyed them. Only one of the 58 subjects responded to this question by mentioning that he felt the outcome might have been rigged. This subject was in the E-Caused Foreknowledge Low Attractive outcome condition, and indicated that he thought the experimenter

Table 9

Means and Standard Deviations for Subjects' Estimates of Perceived Fairness

Attractiveness	Fairness Question		Causality and Foreknowledge: E-Caused Foreknowledge	S-Caused Foreknowledge	S-Caused No Foreknowledge
Low	Fair "to self"	M	13.70[a]	17.00	16.00
		(SD)	(8.22)	(6.75)	(6.99)
	Fair "in general"	M	11.70	13.50	15.30
		(SD)	(7.60)	(6.33)	(7.18)
High	Fair "to self"	M	16.50	16.00	17.00
		(SD)	(4.74	(4.62)	(4.41)
	Fair "in general"	M	17.00	16.50	18.50
		(SD)	(5.38)	(4.50)	(3.51)

[a]For both questions, 0 = "not at all fair"; 20 = "completely fair."

"knew which marble she was picking out." Most of the subjects wrote that nothing had bothered them. A few indicated that they wished they had had free choice, and some commented that they were unhappy with the item they received. However, it never occurred to the vast majority of the subjects that anything was amiss. This suggests that subjects' suspicions concerning the outcome they received were minimal, even in the E-Caused Foreknowledge condition.

On the next page of the questionnaire, subjects were faced with a question that asked specifically whether they ever had the feeling that the outcome was fixed--"that the experimenter somehow cheated to make sure that you got one item instead of the other." The cell means and standard deviations for this question are presented in Table 10. A comparison of the S-Caused Foreknowledge subjects with subjects in the remaining two conditions revealed a significant effect ($F = 3.995$; $df = 1, 52$; $p < .051$), with subjects in this condition viewing the outcome as less "fixed" than subjects in the remaining conditions. There was also a significant effect for outcome ($F = 6.845$; $df = 1, 52$; $p < .012$), with subjects who received a low attractive item regarding the outcome as significantly more "fixed" than subjects receiving a high attractive item. A comparison of the E-Caused Foreknowledge subjects with the S-Caused No Foreknowledge subjects revealed no significant differences, although the interaction of this variable with the attractiveness variable was found to be significant ($F = 6.169$; $df = 1, 52$; $p < .013$). Special contrasts

Table 10

Means and Standard Deviations for Subjects' Feelings That the Outcome Was Fixed

Attractiveness		Causality and Foreknowledge		
		E-Caused Foreknowledge	S-Caused Foreknowledge	S-Caused No Foreknowledge
Low	M	10.60[a]	2.20	4.60
	(SD)	(8.07)	(4.64)	(8.26)
High	M	.20	.80	4.37
	(SD)	(.63)	(1.62)	(8.21)

[a]0 = "no, not at all"; 20 = "very much so."

revealed that, within the Low Attractive outcome condition, the subjects in the E-Caused Foreknowledge cell viewed the outcome as significantly more "fixed" than subjects in the other two cells ($F = 9.491$; $df = 1, 52$; $p < .003$). In the High Attractive outcome condition, there were no significant differences in perceptions of the outcomes being fixed. Taken as a whole, these results suggest that receiving a low attractive outcome that one does not cause is an especially aversive circumstance. Subjects in the E-Caused Low Attractive outcome cell not only concluded that the outcome was "fixed" when that possibility was suggested to them, but also viewed the procedure as relatively unfair, and showed a tendency to derogate the outcome they obtained.

On the "anonymous" questionnaire, subjects were also asked to indicate how much they liked the experimenter. Subjects' responses were relatively high and were not affected by the experimental treatments.

DISCUSSION

The most important finding to emerge from the present study is that merely causing a chance outcome, and having foreknowledge about the consequences, induces feelings of perceived control. It is perhaps surprising that such subtle variables as who picks the marble that determines the chance outcome, and whether he knows what each marble stands for at the time, should influence perceived control. These variables produced strong effects even though subjects were specifically told that the outcome would be determined by chance. The major contribution of the present study, then, is the delineation of two important determinants of perceived control.

The present experiment also provides evidence that perceived control is closely related to feelings of choice and responsibility. Although it seems inherently plausible that most control manipulations might affect subjects' attributions of choice and responsibility as well, these variables have not previously been included as dependent measures. This experiment demonstrates that causality and foreknowledge produce feelings of choice and responsibility as well as feelings of control. It is somewhat remarkable that subjects felt any choice or responsibility at all

for the outcome, since they were told that the outcome was determined by chance. The high intercorrelations between control, choice, and responsibility in the present study suggest the possibility that the effects of perceived control reported in previous studies are mediated by choice and responsibility. The present results clearly suggest that these variables should be taken more seriously by individuals studying controlled and uncontrolled outcomes.

The most surprising thing about the results of this experiment was the failure to find any significant differences between subjects' evaluation of their outcomes in the various experimental conditions. Particularly since a relationship has been found between control, choice, and responsibility, one might expect subjects to evaluate controlled outcomes more favorably than uncontrolled outcomes. There is not even the weakest trend in the data to suggest that this is the case.

Of course, it is important to determine whether there is really no relationship between control over an outcome and evaluation of that outcome, or whether the present experiment was not designed well enough to pick up such a difference. A careful examination of previous studies sheds some light on this problem. Whether control is defined as the ability to perform an instrumental response that terminates the stimulus, access to an "escape button" that will turn off the stimulus, or self-administration of the stimulus, there is virtually no evidence that individuals view outcomes that they "control" more favorably than outcomes

they do not control.

Geer, Davidson, and Gatchel (1970) manipulated control by leading subjects to believe that they had avoided an aversive stimulus by speeding up their reaction time. There were no significant differences in pain thresholds between subjects who had perceived control and subjects who did not. Furthermore, subjects with control did not rate the experience as any less irritating than subjects without control. In a study by Corah and Boffa (1970) and two experiments by Glass and Singer (1972), control was manipulated by informing subjects of an "escape button" and giving them "free choice" about using it. Corah and Boffa found a weak trend when they asked subjects to make discomfort ratings of the shock. However, it has been mentioned earlier that an artifact of the design may have produced this difference. In the two experiments by Glass and Singer, subjects were asked to indicate how distracting, irritating, and unpleasant the noise was. In the first study, subjects who had control rated the noise as significantly less irritating than subjects without control. However, this result was not replicated in the second experiment. Responses for the other adjectives did not reach significance, although subjects with control viewed the noise as marginally less distracting in one study and marginally less irritating in the second one. Even Glass and Singer do not take these differences seriously (see p. 68), and they mention that subsequent experiments in their laboratory have not replicated these effects. In fact, they dismiss a dissonance or Bem-type

explanation for their data by pointing out that a significant difference in the subjective ratings does not occur (see pp. 68-69).

Most of the experiments manipulating who administers the aversive stimulus have included subjective measures, and none have found evidence that control affects subjective ratings of the aversive stimulus. Haggard (1946) found reliable evidence that subjects who administer their own shock show less autonomic arousal than those who do not. He indicates that subjects were also asked to rate "the subjective strength of the shocks." However, he never reported on this measure in his results section, most likely because significant differences were not obtained. Pervin (1963) manipulated whether the shocks were administered by the subject or the experimenter and found that, although subjects preferred S-administered shocks, they did not rate these shocks as less painful than E-administered ones. Staub, Tursky, and Schwartz (1971, Experiment 1) also manipulated who administered the shocks, and found no significant differences in pain thresholds reported by the subjects.[1] Stotland and Blumenthal (1964) exposed subjects to a testing situation, and asked them for subjective ratings of anxiety. They found no significant differences between a group that had some control over the situation and

---

[1]The studies by Staub, Tursky, and Schwartz (1971, Experiment 2) and Pervin (1963) provide evidence that predictability reduces pain thresholds and affects subjective estimates of the aversive stimulus; however, experiments by Haggard (1946) and Glass and Singer (1972) have found no relationship between predictability and subjective evaluation of the aversive stimulus.

a group that did not.

Taken as a whole, these studies suggest that there is no tendency for individuals to evaluate controlled outcomes more favorably than uncontrolled ones. This fact raises a theoretical dilemma of sorts. The results of the present study suggest that feelings of control are closely related to feelings of choice and responsibility. Many years of dissonance research have clearly demonstrated the relationship between choice of and responsibility for an outcome and evaluation of that outcome. Why, then, is there no apparent relationship between perceived control and outcome evaluation?

One possibility is that there is something unusual about the paradigms in which perceived control has been manipulated. What if Glass and Singer had manipulated choice instead of control in one of their experiments? Would the effects of choice be similar to those of control? Would subjects who chose to expose themselves to an aversive stimulus show a "dissonance effect" and rate the stimulus as less aversive than subjects who did not have choice? Glass and Singer (1972) have conducted one experiment dealing with choice. They were interested in determining whether dissonance reduction processes could mitigate the effects of loud noise. They manipulated two variables deemed to be relevant to dissonance theory: whether the noise was necessary (i.e. justified) or unnecessary (i.e. unjustified) and whether subjects had "free choice" about hearing the noise. Necessity was manipulated by informing half of the

subjects that the noise was an integral part of the study and half that it was not. Half of the subjects were told that they would have choice about listening to the noise. The experimenter told them that he would let them hear a burst of noise to help them decide whether they would be willing to listen to the noise. He explained that, unfortunately, not enough subjects had agreed to listen to the noise, so it would really help him if the subject would agree to do so. It was stressed, however, that it was the subject's choice to make, and that he would receive the same money (or credit) regardless of his choice. Subjects in the No Choice condition were simply subjected to the noise bursts without being asked. Following the noise bursts, subjects were asked to complete the proofreading and Stroop tasks, and were asked to give their subjective evaluation of the noise.

Glass and Singer found no significant effects for the necessity variable, and concluded that it may not have been manipulated effectively. However, the results for the perceived choice variable were quite strong. Surprisingly, choice about being exposed to the noise did not affect the subjects' performance on the proofreading task or Stroop test. But Choice subjects reported the noise as significantly less annoying and disruptive of their performance than No Choice subjects. They also were more interested in tasks such as proofreading, and volunteered more time for a later study than subjects in the No Choice condition.

Although Glass and Singer's manipulation of choice is quite similar

to their manipulation of control, the results obtained for perceived choice were quite different. Why should subjects who are told they have "free choice" about stopping the stimulus ("control" manipulation) react differently in the experimental paradigm than subjects given "free choice" about listening to the noises in the first place ("choice" manipulation). If subjects perform better on the post-noise tasks when they have had control over the noise, why don't they perform better when they have choice about hearing the noise?

Taken together, the present study and past ones suggest that perceived control affects behavior differently than does choice, even though perceived control induces feelings of choice. This is a theoretical dilemma that clearly deserves serious attention. What steps might be taken toward resolving this issue? First, it is important that the effects of "choice" and "control" be examined within the same experimental paradigm. Second, factors that might explain the discrepancy between Glass and Singer's results for "control" and "choice," and between the present experiment and "free choice" dissonance experiments, should also be examined. One such factor is the time that has elapsed since an outcome is obtained. Glass and Singer may have obtained different results in their "control" and "choice" studies because the length of time following the aversive stimuli was unwittingly allowed to vary. Likewise, it is possible that a "dissonance" effect was not obtained in the present experiment because the dependent measures were not obtained at the appropriate

time (cf. Walster, 1964).

Suggestions for additional research are also provided by the responses of subjects in the E-Caused condition who received a low attractive outcome. Subjects in this cell tended to view the procedure as unfair, and concluded that the outcomes were "fixed" once that possibility had been suggested to them. They also showed a tendency to derogate the outcome they obtained. These findings suggest that individuals react quite negatively when they receive a relatively unattractive outcome that they do not cause. It is important to determine whether this negative reaction is a reliable one; and, if so, whether it is ameliorated as time passes.

Suggestions for additional research are also provided by the effects that occurred in this study as a result of the foreknowledge variable. It will be recalled that subjects felt control upon picking a marble out of a can only if they knew what each marble stood for at the time. On the surface, it certainly seems unlikely that such a subtle manipulation could affect perceived control. Therefore, it is important to demonstrate that the effects obtained for this variable in the present study are reliable.

An additional experiment manipulating foreknowledge has been conducted, and provides strong support for the assertion that this variable affects perceived control. This experiment was a 2 x 2 factorial design in which foreknowledge and outcome attractiveness were varied. The method was quite similar to the one described previously, except that all

subjects caused their own outcome by picking a marble from a bowl, and the outcomes obtained were foods to be tasted rather than consumer items. Twenty-eight male subjects[1] entered the lab in groups of three. They were told that the purpose of the study was to examine people's attitudes toward food. It was explained that the aim of the project was to reduce the frequency of poor eating habits and malnutrition. After listening to this spiel, each subject went to a private booth where he saw eight foods in front of him. The foods were in individual containers. Half had been selected to be quite attractive (e.g. chocolate torte, Pizza Stix); the remaining foods were selected to be unattractive (e.g. fried grasshoppers, limburger cheese). After inspecting the foods for several minutes, subjects were asked to rate them on a 21-point scale according to how much they liked each one.

Subjects were then given a "filler" task--a questionnaire asking them to rate a variety of food products. While they were working on this task, the experimenter studied their initial ratings and selected a high- and low-rated food to be used as outcomes for each subject. The experimenter collected the filler questionnaire. She then informed the subjects that they would be asked to consider two of the foods in more detail than they had in the initial ratings. She came around and set aside the two

---

[1]Subjects were Duke University students who participated in order to satisfy a course requirement. This experiment was conducted prior to the one described in the Method section.

foods that had been predetermined for each subject as his outcomes. Then she informed the subjects that "an important component of people's attitudes toward food is how much they like their taste," and told them that they would be tasting one of the foods before them. It was explained that they would not be able to choose which food they tasted, since subjects must be "randomly assigned" to conditions. They were told that which food they would taste would be determined by chance--by the color of marble that was picked from a bowl containing two marbles of different colors.

Subjects were then given a bowl containing a red and a blue marble. All subjects were required to reach in without looking and pick out a marble. Subjects in the Foreknowledge condition were told before picking: "If you pick the red marble, you will get to taste the food on your left. If you pick the blue marble, you will get to taste the food on your right." Subjects in the No Foreknowledge condition were told, "After you pick out a marble, please turn over the cards I have put down in front of you. They will tell you which marble stands for which food." Half of the subjects picked the marble that assigned them to the attractive food; the remaining subjects picked the marble assigning them to the unattractive food.

Subjects were then given another "filler" task before making the final ratings of the foods. They were asked to indicate their prior experience with some of the foods. After approximately five minutes had

passed, subjects were asked to rerate the foods, and to complete a questionnaire designed to measure their reactions to the study. The questionnaire asked subjects about perceived control, choice, and responsibility. After completing this questionnaire, subjects were given an opportunity to taste the food if they so desired, but were not required to do so. All subjects were then probed for suspicion and then debriefed.

The means for subjects' feelings of control, choice, and responsibility are reported in Table 11. A two-factor analysis of variance revealed that subjects in the Foreknowledge condition perceived themselves to have significantly more control ($F = 5.740$; $df = 1, 24$; $p < .025$) and significantly more choice about which food they ate ($F = 9.092$; $df = 1, 24$; $p < .006$) than subjects in the No Foreknowledge condition. The analysis for the responsibility question fell short of significance, although the means are clearly in the predicted direction (see Table 11). As in the previous study, there were no effects for the attractiveness variable or interactions between the variables. The means for subjects' evaluations of the obtained and nonobtained foods, and the difference between them, are presented in Table 12. Again, there are no significant effects for the outcome evaluation data, even though subjects in the Foreknowledge condition clearly perceived themselves to have choice about which outcome they received.

Now that it has been established that the foreknowledge variable produces reliable differences in perceived control, it is appropriate to

Table 11

Means for Subjects' Perceptions of Control, Choice, and Responsibility

Attractiveness	Foreknowledge	
	No Foreknowledge	Foreknowledge
Low		
Control	7.00[a]	11.71
Choice	7.00[b]	11.71
Responsibility	10.00[c]	13.86
(N)	(5)	(7)
High		
Control	6.71	17.44
Choice	1.71	12.11
Responsibility	9.57	16.33
(N)	(7)	(9)

[a]Subjects were asked how much they felt they could influence what color of marble they received on a scale ranging from 0 ("not at all") to 31 ("very much").

[b]Subjects were asked how much choice they felt they had about which food they got to taste on a scale ranging from 0 ("no choice at all") to 31 ("complete freedom of choice").

[c]Subjects were asked how responsible they thought they were for the fact that they got to taste "this food" instead of the other one on a scale ranging from 0 ("not at all responsible") to 31 ("completely responsible").

Table 12

Changes in Evaluation of Obtained and Nonobtained Outcomes

Attractiveness	Foreknowledge	
	No Foreknowledge	Foreknowledge
Low		
Obtained	2.00[a]	1.43
Nonobtained	- .20	- .57
Difference	2.20	2.00
High		
Obtained	.43	- .89
Nonobtained	-1.43	- .11
Difference	1.86	- .78

[a]Positive scores indicate increased attractiveness; negative, decreased.

ask why this is the case. One possibility is that, in this condition, subjects make an effort to get a certain marble--they may reach into the dish in a certain way, for example--and this attempt at control leads to feelings of actual control. It would be possible to test this reasoning by varying the difference in attractiveness between the items that the subject has a chance of getting. In the present experiments, there was a good deal of disparity between the items, and subjects in the Foreknowledge conditions may have been making an active effort to obtain a certain marble. If half of the subjects were told that the two marbles stood for items equal in attractiveness, and the remaining subjects were told that one stood for an attractive item and one stood for an unattractive item, only the latter group should make an active effort to obtain a particular marble. If the latter group felt more control over the outcome than the former group, the results would suggest that the attempt to assert control leads to feelings of actual control. Such a finding would be particularly interesting in light of the results obtained by Harvey and Johnson (in press) concerning perceived choice. These investigators found that, when an individual is confronted with two alternatives and asked to make a choice, perceived choice is greater when there is a small difference in attractiveness between the items than when there is a large one.

In addition to the possibilities for research suggested by the present study, several problems mentioned earlier in the paper are in need of attention. Two areas of investigation seem especially promising. First,

a study by Glass and Singer (1972) has suggested that subjects who get an aversive outcome because of their poor performance feel more control over the outcome than subjects who receive an aversive outcome that is not contingent upon their performance. The relationship between failure and perceived control clearly deserves more attention. Second, it is important to obtain more information concerning how individuals react to loss of control. The conditions under which they become angry and assertive, and those under which they become passive and "helpless," clearly need to be specified.

In conclusion, the present research has added to our knowledge concerning control by delineating two important determinants of perceived control, and by demonstrating a relationship between perceived control and feelings of choice and responsibility. When considered in relationship to previous work, the present experiments suggest rather strongly that there is no relationship between perceived control and outcome evaluation. Many questions still remain unanswered, but the results suggest that control is indeed a rich area for further investigation.

APPENDIX I

RAW DATA

Condition	S#	Obtained			Nonobtained			Difference	Subjective Probability	Expectation
		Pre	Post	Ch	Pre	Post	Ch			
E-Caused Foreknowledge Low										
	1	4	4	0	17	17	0	0	.50	none
	2	6	9	+3	16	13	-3	+6	.50	low
	3	5	5	0	17	16	-1	+1	.50	low
	4	3	0	-3	13	13	0	-3	.50	none
	5	4	3	-1	15	16	+1	-2	.50	none
	6	6	2	-4	18	19	+1	-5	.50	low
	7	8	1	-7	18	17	-1	-6	.50	low
	8	4	1	-3	16	19	+3	-6	.50	low
	9	3	1	-2	17	17	0	-2	.60	low
	10	0	0	0	17	18	+1	-1	.40	low
E-Caused Foreknowledge High										
	11	15	15	0	2	3	+1	-1	.50	none
	12	15	16	+1	6	6	0	+1	.50	none
	13	14	14	0	3	3	0	0	.50	none
	14	18	18	0	3	1	-2	+2	.50	high
	15	18	19	+1	2	0	-2	+3	.50	high
	16	14	13	-1	5	6	+1	-2	.30	low
	17	18	19	+1	7	2	-5	+6	.75	high
	18	18	18	0	5	12	+7	-7	.99	high
	19	16	7	-9	4	1	-3	-6	.50	none
	20	17	15	-2	4	6	+2	-4	.50	high

Condition	S#	Control	Choice	Responsi-bility	Fair for Self	Fair in General	Outcome Fixed	Like Experimenter
E-Caused Foreknowledge Low								
	1	0	0	0	20	20	0	16
	2	0	0	0	18	20	15	11
	3	0	0	0	20	20	0	11
	4	0	0	0	10	10	20	11
	5	0	0	0	0	0	20	18
	6	0	0	0	0	0	14	20
	7	0	0	0	20	10	0	9
	8	0	0	5	10	10	17	20
	9	17	0	0	19	17	10	18
	10	0	3	15	20	10	10	12
E-Caused Foreknowledge High								
	11	0	0	0	20	20	0	10
	12	0	0	0	20	20	0	19
	13	0	0	0	15	15	0	18
	14	0	0	0	5	5	0	10
	15	0	0	0	20	20	0	18
	16	0	0	0	20	20	0	10
	17	0	0	0	20	20	2	10
	18	20	0	0	20	20	0	20
	19	0	0	0	20	20	0	15
	20	0	0	0	10	10	0	5

Condition	S#	Obtained			Nonobtained			Difference	Subjective Probability	Expectation
		Pre	Post	Ch	Pre	Post	Ch			
S-Caused Foreknowledge Low										
	21	2	2	0	18	18	0	0	.50	high
	22	5	2	-3	15	15	0	-3	.50	none
	23	1	0	-1	17	15	-2	+1	.02	low
	24	9	1	-8	16	17	+1	-9	1.00	high
	25	3	2	-1	15	13	-2	+1	.50	high
	26	7	18	+11	17	18	+1	+10	.80	high
	27	3	1	-2	18	20	+2	-4	.25	low
	28	4	7	+3	17	17	0	+3	.40	low
	29	5	2	-3	13	19	+6	-9	.50	high
	30	3	7	+4	15	18	+3	+1	.50	low
S-Caused Foreknowledge High										
	31	15	18	+3	3	8	+5	-2	.50	high
	32	12	8	-4	1	2	+1	-5	.40	low
	33	11	6	-5	3	8	+5	-10	L*	high
	34	15	12	-3	4	0	-4	+1	.50	low
	35	16	15	-1	3	2	-1	0	.50	high
	36	11	9	-2	3	2	-1	-1	.50	none
	37	12	10	-2	4	4	0	-2	.50	low
	38	16	18	+2	4	3	-1	+3	.33	low
	39	15	16	+1	5	6	+1	0	.50	low
	40	15	12	-3	6	10	+4	-7	.60	high

*Left blank.

Condition	S#	Control	Choice	Responsi-bility	Fair for Self	Fair in General	Outcome Fixed	Like Experimenter
S-Caused Foreknowledge Low								
	21	18	18	18	10	10	15	12
	22	0	20	20	20	20	0	18
	23	20	15	20	20	15	2	17
	24	0	5	6	0	10	0	18
	25	0	0	0	20	3	3	20
	26	14	10	4	20	7	0	14
	27	0	0	20	20	20	0	20
	28	9	10	0	20	10	2	19
	29	3	4	0	20	20	0	16
	30	1	0	0	20	20	0	20
S-Caused Foreknowledge High								
	31	0	10	10	20	20	0	15
	32	2	10	5	20	20	0	16
	33	15	10	8	10	8	1	8
	34	0	6	6	10	10	5	16
	35	7	7	10	10	17	0	14
	36	1	1	2	20	20	0	10
	37	10	11	15	20	20	0	20
	38	7	2	0	16	16	0	9
	39	0	10	0	20	20	0	15
	40	6	6	9	14	14	2	17

Condition	S#	Obtained			Nonobtained			Difference	Subjective Probability	Expectation
		Pre	Post	Ch	Pre	Post	Ch			
S-Caused										
No Foreknowledge										
Low										
	41	4	2	-2	13	7	-6	+4	.50	low
	42	2	2	0	15	15	0	0	.50	none
	43	3	3	0	14	13	-1	+1	.33	low
	44	2	2	0	14	16	+2	-2	.50	low
	45	2	0	-2	17	15	-2	0	.50	low
	46	4	4	0	12	13	+1	-1	.50	none
	47	3	4	+1	15	12	-3	+4	.50	none
	48	3	11	+7	15	16	+1	+6	.50	low
	49	2	8	+6	13	16	+3	+3	.40	low
	50	5	4	-1	20	20	0	-1	.50	low
S-Caused										
No Foreknowledge										
High										
	51	14	13	-1	2	3	+1	-2	.50	high
	52	15	15	0	4	2	-2	+2	.75	high
	53	16	18	+2	5	4	-1	+3	.50	none
	54	17	18	+1	5	3	-2	+3	.50	low
	55	18	16	-2	5	12	+7	-9	.50	low
	56	14	16	+2	2	6	+4	-2	.75	high
	57	17	15	-2	5	3	-2	0	.50	high
	58	17	15	-2	1	2	+1	-3	.50	none

Condition	S#	Control	Choice	Responsi-bility	Fair for Self	Fair in General	Outcome Fixed	Like Experimenter
S-Caused								
No Foreknowledge								
Low								
	41	0	0	0	20	16	20	14
	42	7	0	10	10	7	5	7
	43	0	0	0	0	0	20	18
	44	0	0	0	20	20	0	16
	45	0	0	0	10	10	0	14
	46	0	0	0	20	20	0	15
	47	0	0	0	20	20	0	19
	48	0	0	0	20	20	0	20
	49	0	0	0	20	20	1	13
	50	0	0	0	20	20	0	15
S-Caused								
No Foreknowledge								
High								
	51	17	10	13	20	10	0	6
	52	12	10	11	10	20	15	19
	53	0	0	0	20	20	0	16
	54	5	3	1	18	20	0	20
	55	0	0	0	18	18	0	15
	56	0	0	0	10	20	20	10
	57	0	0	0	20	20	0	18
	58	0	0	0	20	20	0	20

APPENDIX II

DEPENDENT MEASURES

Name_____

1. What were the two items that the experimenter told you that you had a chance of getting?

_____ and _____

2. Which item did you hope you would get? _____

3. What did you think <u>your</u> chances were of getting the item you wanted most? (From 1 to 100%) _____

4. Which item did you expect to get? _____

5. How much did you feel that you could influence what color of marble you received?

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
not at all very much

6. Which consumer item did you get? _____

7. How much choice do you feel you had about which item you got?

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
no choice at all complete freedom of choice

8. How responsible would you say you are for the fact that you got this item instead of the other one?

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
not at all responsible completely responsible

9. How fair do you think it is that you got this item instead of the other one?

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
not at all fair completely fair

10. In general, how fair would you say the procedure is for deciding who gets which item?

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
not at all fair completely fair

Directions:

On this questionnaire, we would like you to evaluate the young lady who conducted this experiment. The people who sponsor this research hire a number of different experimenters to help them, and they like to find out from the students who participate how well the experimenters are doing. When you have completed this questionnaire, please seal it in the envelope and place it in the box by the door as you leave. Your evaluation will be strictly confidential, and it will not be seen by the experimenter. Therefore, we would like you to be as honest as possible in your evaluation. We have also included a few questions about your opinion of the experiment. Your responses will be very helpful to us in improving this study in the future.

1. At the beginning of the experiment, how well did the experimenter do at describing the purpose of the research to you?

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
very poorly very well

2. Was the experimenter efficient in handing out and collecting materials for the experiment?

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
not at all efficient very efficient

3. Please think back to the part of the experiment in which the experimenter told you that you would receive a consumer item, and that the item you received would depend on the color of the marble you got. Was there anything about this part of the study that bothered you? If so, what?

Did the experimenter do or say anything during this part of the study that annoyed you?

4. Please think back to the part of the experiment in which the marble was picked. Did you ever have the feeling that the outcome was fixed--that the experimenter somehow cheated to make sure that you got one item instead of the other?

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
no, not at all — very much so

5. Overall, how well did the experimenter do in conducting the study?

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
very poorly — very well

6. How much did you like the experimenter personally?

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
not at all — very much

7. All in all, how much did you enjoy participating in this experiment?

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
not at all — very much

8. How important do you think the results of this experiment are likely to be?

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
not at all important — very important

Directions:

Please rate each of the consumer items according to how much you like it. Indicate your rating by circling a number on each scale on the next page.

We are also interested in finding out whether you own any of these items. Do you own any items that are exactly like these, or very similar to them? If so, please write the numbers of the items that you own on the bottom of this page. If you do not own any of these items, leave the bottom of this page blank. If you have any questions, please raise your hand, and the experimenter will help you.

Numbers of the items that you own (if any):

Name_____ Age_____

Item #1:

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
not at all slightly moderately pretty much very much

Item #2:

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
not at all slightly moderately pretty much very much

Item #3:

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
not at all slightly moderately pretty much very much

Item #4:

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
not at all slightly moderately pretty much very much

Item #5:

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
not at all slightly moderately pretty much very much

Item #6:

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
not at all slightly moderately pretty much very much

Item #7:

0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
not at all				slightly					moderately					pretty much					very much	

Item #8:

0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
not at all				slightly					moderately					pretty					very much	

OPINIONS ABOUT CURRENT ADVERTISEMENTS

Name_____

Directions:

We would like you to rate some current advertisements for us according to how good you think they are. By a good ad, we mean one that captures your attention and leads you to feel favorably toward the product. The ads that we would like you to rate are in the accompanying issue of Life magazine. Please look up each ad listed below and then rate it on the scale that has been provided. Do not spend too much time looking at any one ad--just glance at it the way you would if you were reading the magazine, and then make your rating.

1. Chevrolet ad (page 1)

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
very poor ad — very good ad

2. Allstate ad (page 2)

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
very poor ad — very good ad

3. Navy ad (page 13)

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
very poor ad — very good ad

4. Benson & Hedges 100's ad (pages 14 and 15)

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
very poor ad — very good ad

5. American Gas Association ad (page 17)

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
very poor ad — very good ad

6. Sports Illustrated ad (page 19)

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
very poor ad — very good ad

7. Haverhill's ad (page 20)

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
very poor ad — very good ad

8. Smirnoff ad (page 24a)

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
very poor ad — very good ad

Name_____

Directions:

What we would like you to do now is to rate the 8 consumer items for us again. This may seem like a strange request, since you were asked to rate them at the beginning of the experiment, which was not too long ago. However, we are interested in how people's opinions of consumer items change as they become more familiar with the items. Some researchers think that the more often we see a certain consumer item, the more we will come to like it. Others feel that just the reverse is true; that initially we may like an item quite a bit but later notice all the things that are wrong with it.

In order to investigate this question, we would like you to examine each of the consumer items again, and rate them using the scales on the next page. Now that you are more familiar with these items, you may like some of them better than you did initially; and you may not like some items as well as you did before. Or, your feelings about the items may be just the same as they were before. Please take a moment or two to re-examine the items, and then rate them according to how you feel about them now. Indicate how much you like each item by circling a number on the scale.

Item #1:

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
not at all slightly moderately pretty much very much

Item #2:

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
not at all slightly moderately pretty much very much

Item #3:

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
not at all slightly moderately pretty much very much

Item #4:

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
not at all slightly moderately pretty much very much

Item #5:

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
not at all slightly moderately pretty much very much

Item #6:

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
not at all slightly moderately pretty much very much

Item #7:

0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
not at all				slightly					moderately						pretty much				very much	

Item #8:

0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
not at all				slightly					moderately						pretty much				very much	

REFERENCES

Allen, V. Uncertainty of outcome and post-decision dissonance reduction. In L. Festinger, Conflict, decision, and dissonance. Stanford: Stanford University Press, 1964. Pp. 34-44.

Aronson, E. Dissonance theory: Progress and problems. In R. P. Abelson, E. Aronson, W. J. McGuire, T. M. Newcomb, M. J. Rosenberg, & P. H. Tannenbaum (Eds.), Theories of cognitive consistency: A sourcebook. Chicago: Rand McNally, 1968. Pp. 5-27.

Badia, P., McBane, B., Suter, S., & Lewis, P. Preference behavior in an immediate versus variably delayed shock situation with and without a warning signal. Journal of Experimental Psychology, 1966, 72, 847-852.

Brame, D. Dissonance, expectation, and the self. In R. P. Abelson, E. Aronson, W. J. McGuire, T. M. Newcomb, M. J. Rosenberg, & P. H. Tannenbaum (Eds.), Theories of cognitive consistency: A sourcebook. Chicago: Rand McNally, 1968. Pp. 355-366.

Brehm, J. W. A theory of psychological reactance. New York: Academic Press, 1966.

Brehm, J. W. Responses to loss of freedom: A theory of psychological reactance. New York: General Learning Press, in press.

Bresnahan, J. L., & Shapiro, M. M. A general equation and technique for the exact partitioning of chi-square contingency tables. Psychological Bulletin, 1966, 66, 252-262.

Buresh, B. Profile of an assassin. Newsweek, 1972, 79, 24.

Corah, N. L., & Boffa, J. Perceived control, self-observation, and response to aversive stimulation. Journal of Personality and Social Psychology, 1970, 16, 1-14.

De Charms, R. Personal causation: The internal affective determinants of behavior. New York: Academic Press, 1968.

Farrell, B. New way to run the big house: Washington State Penitentiary's inmate councils. Life, 1972, 73, 32-37.

Freud, S. Beyond the pleasure principle. (Trans. by J. Strachey, 1950) New York: Liverwright, 1920.

Fromm, E. The Erich Fromm theory of aggression. New York Times Magazine, February 27, 1972.

Geer, J. H., Davidson, G. C., & Gatchel, R. I. Reduction of stress in humans through nonveridical perceived control of aversive stimulation. Journal of Personality and Social Psychology, 1970, 16, 731-738.

Glass, D. C., Reim, B., & Singer, J. E. Behavioral consequences of adaptation to controllable and uncontrollable noise. Journal of Experimental Social Psychology, 1971, 7, 244-257.

Glass, D. C., & Singer, J. E. Urban stress. New York: Academic Press, 1972.

Goffman, E. Interaction ritual. New York: Anchor Books, 1967.

Haggard, E. A. Some conditions determining adjustment during and readjustment following experimentally induced stress. In S. S. Tomkins (Ed.), Contemporary psychopathology. Cambridge, Mass.: Harvard University Press, 1946.

Harvey, J. H., & Johnson, S. Determinants of the perception of choice. Journal of Experimental Social Psychology, in press.

Hokanson, J. E., DeGood, D. E., Forrest, M. S., & Brittain, T. M. Availability of avoidance behavior in modulating vascular-stress responses. Journal of Personality and Social Psychology, 1971, 19, 60-68.

Janis, I. Air war and emotional stress. New York: McGraw-Hill, 1951.

Janis, I. L. Psychological stress. New York: John Wiley, 1958.

Jecker, J. D. The cognitive effects of conflict and dissonance. In L. Festinger, Conflict, decision, and dissonance. Stanford: Stanford University Press, 1964. Pp. 21-32.

Kelley, H. H. Attribution in social interaction. New York: General Learning Press, 1971.

Kelly, G. A. The psychology of personal constructs. New York: Norton, 1955.

Kubler-Ross, E. On death and dying. New York: Macmillan, 1969.

Le Panto, R., Moroney, W., & Zehnhausern, R. The contribution of anxiety to the laboratory investigation of pain. Psychonomic Science, 1965, 3, 475.

Lerner, M. J. The desire for justice and reaction to victim. In J. Macauley & L. Berkowitz, Altruism and helping behavior. New York: Academic Press, 1970. Pp. 205-229.

Lerner, M. J. Deserving versus justice: A contemporary dilemma. Report #24. University of Waterloo, 1971.

Lerner, M. J., & Simmons, C. Observer's reaction to the innocent victim: Compassion or rejection? Journal of Personality and Social Psychology, 1966, 4, 203-210.

Lewin, K., Dembo, T., Festinger, L., & Sears, P. Level of aspiration. In J. McV. Hunt (Ed.), Personality and the behavior disorders. New York: Ronald Press, 1944. Pp. 333-378.

Malinowski, B. Magic, science, and religion. New York: Free Press, 1948.

Orne, M. T. On the social psychology of the psychological experiment: With particular reference to demand characteristics and their implications. American Psychologist, 1962, 17, 776-783.

Pervin, L. A. The need to predict and control under conditions of threat. Journal of Personality, 1963, 31, 570-587.

Richter, C. On the phenomenon of sudden death in animals and man. Psychosomatic Medicine, 1957, 19, 191-198.

Roth, S., & Bootzin, R. The effects of experimentally induced expectancies of external control: An investigation of learned helplessness. Journal of Personality and Social Psychology, in press.

Schultz, T. Rape, fear, and the law. Chicago Guide, November 1972, 21, 56-62.

Seligman, M. E. P. Depression and learned helplessness. Unpublished manuscript, University of Pennsylvania, 1970.

Seligman, M. E. P., & Maier, S. F. Failure to escape traumatic shock. Journal of Experimental Psychology, 1967, 74, 1-9.

Seligman, M. E. P., Maier, S. F., & Geer, J. The alleviation of learned helplessness in the dog. Journal of Abnormal Psychology, 1968, 73, 256-262.

Seligman, M. E. P., Maier, S. F., & Solomon, R. L. Unpredictable and uncontrollable aversive events. In F. R. Brush, Aversive conditioning and learning. New York: Academic Press, 1971. Pp. 347-400.

Staub, E., Tursky, B., & Schwartz, G. E. Self-control and predictability: Their effects on reactions to aversive stimuli. Journal of Personality and Social Psychology, 1971, 18, 157-162.

Stotland, E., & Blumenthal, A. The reduction of anxiety as a result of the expectation of making a choice. Canadian Journal of Psychology, 1964, 18, 139-145.

Streufert, S., & Streufert, S. C. Effects of conceptual structure, failure, and success on attribution of causality and interpersonal attitudes. Journal of Personality and Social Psychology, 1969, 11, 138-148.

Strickland, L. H., Lewicke, R. J., & Katz, A. M. Temporal orientation and perceived control as determinants of risk-taking. Journal of Experimental Social Psychology, 1966, 2, 143-151.

Taulbee, E. S., & Wright, H. W. A psychosocial-behavioral model for therapeutic intervention. In C. D. Spielberger (Ed.), Current topics in clinical and community psychology, III. New York: Academic Press, 1971.

Thibaut, J. W., Coules, J., & Robinson, E. The effects of perceived control on acceptance. In J. W. Thibaut & H. W. Riecken, Authoritarianism, status, and the communication of aggression. Human Relations, 1955, 8, 95-120.

Walster, E. The temporal sequence of post-decision processes. In L. Festinger, Conflict, decision, and dissonance. Stanford: Stanford University Press, 1964. Pp. 112-128.

Walster, E. "Second guessing" important events. Human Relations, 1967, 20, 239-250.

White, R. W. Motivation reconsidered: The concept of competence. Psychological Review, 1959, 66, 297-333.

Woodworth, R. S. Dynamics of behavior. New York: Holt, 1958.

Worchel, S. Reactance and the frustration-aggression relationship. Unpublished major area paper, Duke University, 1969.

Worchel, S. The effect of simple frustration, violated expectancy, and reactance on the instigation to aggression. Unpublished doctoral dissertation, Duke University, 1971.

Zimbardo, P. G. The human choice: Individuation, reason, and order versus deindividuation, impulse, and chaos. In W. J. Arnold & D. Levine, Nebraska Symposium on Motivation. Lincoln: University of Nebraska Press, 1969. Pp. 237-307.

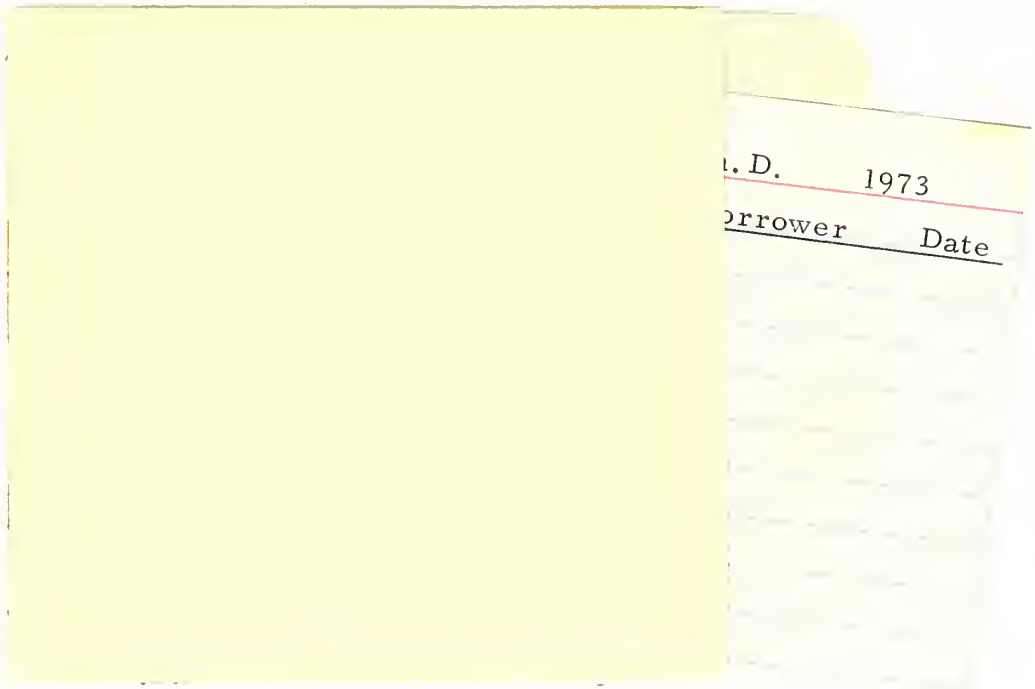
Zimbardo, P. G. Cognitive dissonance and the control of human motivation. In R. P. Abelson, E. Aronson, W. J. McGuire, T. M. Newcomb, M. J. Rosenberg, & P. H. Tannenbaum (Eds.), Theories of cognitive consistency: A sourcebook. Chicago: Rand McNally, 1968. Pp. 439-448.

BIOGRAPHY

The author was born in Pittsburgh, Pennsylvania, on July 2, 1947. Her undergraduate education began at Carnegie-Mellon, where she worked as an assistant for Dr. Daryl J. Bem. She completed her undergraduate training at Duke University and graduated in 1969 with distinction in psychology. She then began graduate training in the Social Psychology program at Duke University. In 1971, the author received a National Institute of Mental Health fellowship. In September, 1972, she accepted a job at Northwestern University as an Assistant Professor of Psychology. Her research interests include perceived control, learned helplessness, and attribution of responsibility. She has published the following articles:

Linder, D. E., Wortman, C., & Brehm, J. W. Temporal changes in predecision preferences among choice alternatives. Journal of Personality and Social Psychology, 1971, 19, 282-284.

Wortman, C., Costanzo, P., & Witt, T. The effect of anticipated performance on the attribution of causality to self and others. Journal of Personality and Social Psychology, in press.


. D. 1973
orrower Date